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Psychological Monographs

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Committee:

Charles Hubbard Judd
Walter B. Pillsbury
Carl E. Seashore
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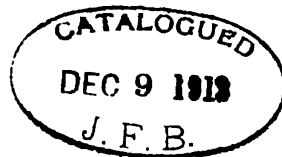
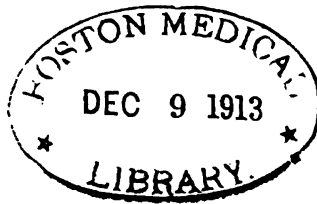
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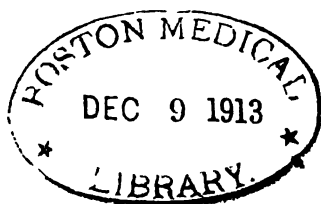
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PREFACE

It has often been felt that the American Psychological Association ought actively to undertake, as well as merely to encourage, systematic contributions to research and the increase of scientific knowledge. In response to this sentiment the Association at its meeting held in New York City, December 27, 28 and 29, 1906, authorized the "creation of a permanent committee of the Association, to consist of five members, which shall act as a general control committee on the subject of measurements. It is recommended that this committee undertake two general lines of work, organizing as many subcommittees as it shall see fit, and calling to its assistance such outside help as it may desire: first, the determination of a series of group and individual tests, with reference to practical application, and second, the determination of standard experiments of a more technical character."¹

The present report is the work of the committee appointed in execution of this resolution. The subjects assigned for investigation to the several members of the committee will be found representative of the two main lines of inquiry approved by the Association. They are as follows: C. H. Judd, tests on motor activities; W. B. Pillsbury, determination of intensity of sound; C. E. Seashore, discrimination of pitch; R. S. Woodworth, difference threshold in color tone, and free and controlled association; J. R. Angell, determination of mental imagery.

In accordance with the provisions of the resolution already partially quoted, the committee invited Professors R. M. Yerkes and J. B. Watson to report on tests for color vision in animals. Professor Judd invited the assistance of Professor

¹ A somewhat similar committee on physical and mental tests was appointed by the Association in 1895, and reported its recommendations at the annual meeting held at Boston and Cambridge in 1896.

Raymond Dodge in formulating tests on eye movement in connection with the study of nervous fatigue and mental disease. Professor Woodworth secured the coöperation of Dr. Lyman Wells in his work on association, and Professor Angell has been assisted in his work by Dr. M. R. Fernald.

The publication of the reports has been delayed much beyond original expectation because the work has proved unexpectedly arduous and difficult. Nevertheless, the committee has made explicit reports of progress at each of the annual meetings of the association since its appointment, and Professor Dodge's work after being thus reported has already appeared, being finally published in conjunction with Professor Diefendorf.²

If the funds appropriated to the purpose had permitted, the committee would probably have chosen to print all their material under one cover. This was not practicable, however, and the present method is accordingly accepted, despite its drawbacks. The reports thus far arranged for are to appear as follows: In the present volume—

Methods for Determination of Intensity of Sound, W. B. Pillsbury.

Measurement of Pitch Discrimination, C. E. Seashore.

Determination of Mental Imagery, J. R. Angell.

In the Animal Behavior Monographs, Yerkes' and Watson's report on tests of the color vision of animals.

In the monographs of the Psychological Review, Woodworth's and Wells' report on association tests.

Professor Judd's work on motor tests has been unavoidably interrupted and it is not possible to speak with certainty of its appearance.

None of these reports is to be considered final. Not only are there outstanding issues to be further studied, but after the reports have been subjected to criticism and practical tests by members of the association and other psychologists, the committee hopes, if continued in office, to print a more authoritative supplementary report in which the final judgment

² *Experimental Study of the Ocular Reactions of the Insane from Photographic Records*, Brain, 1908, Vol. 31, pp. 451-492.

of the committee as a whole will be registered in favor of one or another form of apparatus, method of procedure, etc. The committee will welcome all specific criticisms and suggestions.

I

METHODS FOR THE DETERMINATION OF THE INTENSITY OF SOUND

W. B. PILLSBURY

It seems to be the function of a report of this kind to consider the general principles that govern the problem, to compare and criticize the various methods that have been used and suggested, and to offer suggestions based on tests of the different methods available.

The object of any device to measure the sensitivity of the ear must be to produce a vibration of the tympanic membrane of measurable amplitude or intensity. The intensity of the sound wave at the ear will depend upon two general factors: the intensity of the sound at the sounding body and the laws that govern the distribution of the sound waves about that source. To know how intense the sound will be at the ear one must know how great the amplitude of vibration is at the sounding body, and the distance from the ear together with the way in which the sound spreads from the source, or one must have some means of measuring the intensity of the tone at the ear itself. A complete discussion involves: (1) A study of the different methods of inducing sounds and of measuring the intensity of the sounds that are produced. (2) A study of the laws that control the distribution of sound waves and their intensity at different distances. (3) Or one must devise some methods of measuring the intensity of the tone at the ear.

Perhaps the method of inducing a sound most used is by falling bodies, either freely falling or by the pendulum. Practically all of the more important psychological investigations have been carried on by that method. It is desirable therefore to determine as carefully as possible the controls that are necessary in this method and the means of measuring

the intensity of the tone. This is the more important, as there has been some controversy in the literature over the relation between the height of fall and intensity. Starke asserts that the intensity is dependent upon the product of the mass and the height, Vierordt that it varies as mass times the height raised to the exponent k and k for him is 0.6. Tischer came to the opinion that k varies with the height and with the mass and that in consequence no law may be asserted. Fechner also expressed the opinion that k was variable and must be determined for each different substance of ball and plate.

Obviously the first problem is to determine the relation of the intensity to the height of fall. I attempted this in three ways, but the simplest and only one that was carried through at all successfully was by measuring the amplitude of vibration excited in a fork by the impact of a rubber ball falling from different heights. The results may be seen in the curves that are published herewith (fig. 1). In general it will be seen that the amplitude increases much more rapidly than the square root of the height. Each set was taken on different days. The amplitude was measured directly with a reading microscope.

To show the relation, the square roots of the heights have been plotted below the curve plotted from the amplitudes of vibration measured directly with the microscope. Each point is the average of from fifteen to thirty determinations. The mean variations were from .2 to .5 scale divisions. They are stated at the bottom of the curve for each point. It will be seen that the curve of amplitude of vibration lies between the curve of the square roots and a straight line. As the intensity of the sound is proportional to the square of the amplitude of vibration this means that the sound increases more rapidly than the height but less rapidly than the square of the height. It should be added of course that this result will not necessarily hold for any other ball or any other receiving surface. If the work in the determination of Weber's law in the Leipzig laboratory had been made with this fork, the result would be quite different from the formulae given in the

literature. What the actual formula for their apparatus is no one can know now.

In order more nearly to approximate the conditions of use of the fall phonometer, I attempted to measure with Wien's resonator one component of the noise made by dropping balls of wood and hard rubber upon different surfaces. The surfaces chosen were not particularly suitable to give perfect tones, but were some objects about the laboratory large enough to give the deep tone of the resonator, 120 VD. One was an ordinary wooden box, the other a galvanized iron basket. They were always placed upon a spring couch padded with cloth to prevent transmission of the tone through the floor or other solid objects. Of course there were other tones present than the one measured, and it is possible that the different components were favored by the different heights, but uncertainty in the quality of a tone is nearly as grave as uncertainty in intensity. The results are given in figs. 2 and 3. It will be seen that with conditions as nearly alike as they could be made from day to day there are enormous differences both in the absolute intensities of the tone and in the character of the curve. Some show practically a straight line or an increase in proportion to the square of the height, others are more nearly proportional to the height. These statements both rest upon the assumption that the intensity of the tone is proportional to the square of the rough measurements of the deflections seen in the manometer apparatus. Each value in the curve is the average of five readings taken in immediate succession. The irregularity of the different readings is probably responsible for the irregular fluctuations in certain parts of the curves. It may be argued that my materials are not to be compared with the ebony or ivory or steel plate that is ordinarily used in the fall phonometer. This I would readily grant, but would insist that everything that we know of vibrating plates anywhere indicates that the arrangement of nodes depends upon the point of the plate that is struck and the quality of the tone depends upon the arrangement of the nodes. Then too there is nothing in the character of the board in the bottom of our box that would make the

tone change from day to day, that is not also found in the plates ordinarily used. The curve of sound intensity has never been worked out for an instrument of that sort and until it is, one can know nothing of the sound values that are represented by the height of fall.

The explanation of the disparities is to be conjectured only, but every source of error that we had to contend with is represented in the actual experiment in which the ear replaces the resonator. First any plate vibrates in segments and has different partial vibrations. Occasionally variations in a single result were undoubtedly due to the weakness of the component that we were measuring. The pitch of the complex noise would be noticeably different when the reading was found to be unusually low or high. Anyone who has worked with the ordinary ebony plate fall apparatus will recall that frequently a tone will be very different in intensity and quality from the usual ones. The well known fact that the same intensity induces different effects at different pitches would probably suffice to account for the different apparent intensities of these tones. In addition to the different conditions of reflection, the presence of different bodies and the position of the resonator with reference to the walls undoubtedly plays a very important part. One of these came to our notice in this experiment. It had grown close and a door was opened very slightly. The opening was two meters or more from the source of sound and about the same distance away from the line between the source and the resonator. Nevertheless the readings at once dropped very markedly and continued lower until the door was closed. All of these irregularities indicate that while the relation of intensity to height of fall is fairly close in the tuning fork, that for the apparatus that is ordinarily used the variations are too large and irregular to enable one to obtain results that shall be constant, to say nothing of obtaining absolute measurements. Since the pendulum implies all of the fundamental principles of the freely falling balls, in addition to a source of error from the tone of the suspending rod, it is unquestionably equally unreliable.

Attempts have been made to determine the absolute limen by the use of falling particles. It is assumed that the energy of the falling body may all be transformed into sound and that the product of the height and the mass will indicate the energy that affects the ear provided suitable corrections are made for the distance from the ear. Theoretically this is open to two objections. Certainly not all of the energy is transformed into sound. Much is lost in heating the ball and the plate. How much is left can only be a matter of conjecture. Estimates of the energy transformed into sound under other conditions vary from $1/1000$ for the telephone to $1/15$ that Wead estimated for this tuning forks. Webster's more recent estimates for a more economical transformer of energy than the ball and falling plate would place the loss nearer the higher figure. At the most the energy computations would give only the maximum values and whether the true limen were $1/10$ or $1/1000$ of that no one is at present in position to say. In addition to this disadvantage of not knowing what the results mean, the sound produced is open to all of the variations that the larger ball was seen to show in the experiment just described. Early in this investigation an attempt was made to calibrate a Lehmann acoumeter by determining the distance at which a small ball of cork or pith might be heard when it fell different heights. No regular results could be obtained. First the different reflections at different positions in the room made it impossible to draw any results from the distance. The sound might be fully as intense or even more intense at the more remote as at the nearer distance. The same height of fall often gave widely different results. This was undoubtedly due to the relation of the position of impact to the nodes of the plate. Combined, these factors made the method absolutely valueless. The theoretical and practical disadvantages combined make the falling body method worthless even for student exercises.

A second method is the tuning-fork method that Wead devised and which in a modified form is a test widely used by the acoustician. It is much more promising theoretically since it offers a more accurate measure of the absolute inten-

sity and is more generally applicable. Wead's¹ method was to measure the amplitude of vibration of the fork and to calculate the energy of the vibration from the dimensions of the fork and Young's modulus for steel. He tested the damping of the vibrations and the weight necessary to bend the fork and found that the formula held in practice with surprising accuracy. To determine the potential energy in the König Ut₂ fork the following formula suffices. The potential energy in each prong equals $23.94 \times 10a$ Ergs.² In the actual experiments a (the amplitude of vibration) is read directly by a microscope and the energy is computed from the formula. The experimenter takes the reading at the instant the sound disappears, and the intensity for the ear is computed from the energy at the fork and the distance of the observer.

Two difficulties may be pointed out in connection with the method. First the absolute energy transformed into sound waves is not known, and cannot be accurately separated from the energy spent in heating the fork and in communicating tremors to the supports. Wead had some results that indicate that 1/15 was transformed into sound but did not regard the results as conclusive. The second source of error is even greater. It is the difficulty in determining how the sound will be distributed. Even in the open the ground will reflect or absorb the energy differently, and in a closed room it is practically impossible to say what the law of distribution is. There will be nodes due to reflection from the walls and other interferences and reinforcements that cannot be determined. In the open the wind will make a considerable difference in the distribution. In spite of the disadvantages the results obtained by the method agree fairly well with those obtained by other methods as will be seen when the values that have been determined are reported.

The general formula is

$$Y = \frac{b d_r^3 E}{8l^3} a^2$$

¹ Wead: *Am. Journal of Science*, Se. 3, Vol. 26, p. 177.

v = energy of fork; b = width, d = thickness, l = length of fork, and E = Young's modulus.

For psychologists the method has the added advantage of using instruments that are everywhere available. Every laboratory has a tuning-fork and a microscope and that is practically all that is necessary. As compared with the fall phonometer or sound pendulum it gives a tone of known pitch and of constant pitch. The variation of sensitivity with pitch makes any attempt to use noise uncertain. Then too the constancy of pitch will obviate the uncontrollable pitch variations of the block that were seen to make the free falling ball method valueless. Other instruments have the further disadvantage that one cannot change the intensity at will except by changing the distance from the ear.

The ear specialists use another modification of the tuning-fork method that has some advantages. Instead of measuring the amplitude directly they measure the time that the fork may be heard by a defective ear and compare it with the time that the same tone is heard by a normal ear. It has been determined that the amplitude of vibration of the fork at two times will be proportional to e^{-ht} in which e is the base of the natural logarithm, t is the time during which the tone is heard and h a constant that depends upon the rate of damping of the fork. This must be determined for each fork. If t is the time during which the fork is heard by the ear to be investigated, and t_n the time it will be heard by the normal ear, the limen H is expressed by the equation $H_p = e^{-2h(t_n - t_h)} H_n$. It might be objected that one of these measurements will always be subjective. To this one can reply that the variation in the normal ear is probably less than the variation in the objective conditions under the most favorable circumstances. If the two ears be placed exactly in the same place the space error of the distant stimulus will be avoided which should fully compensate for any change in the sensitivity of the standard ear.¹ It is also possible to determine the absolute sensitivity of the normal ear.

Y

¹ Schaefer: Nagel's *Handbuch d. Physiologie*, Vol. III, pp. 493-495.

One of the most convenient instruments for determining the limen of hearing or for making any tests that require absolute values is the telephone. The telephone may be easily changed in pitch or in intensity and very slight gradations of the tone may be obtained. The convenience of the telephone in use rests mainly upon the fact that the intensity of its tone varies directly with the current and that the current may be measured when the sound itself is very weak. In practice it is necessary merely to establish the relation between a given current and some measurable intensity of the tone and then to measure the strength of the current when the tone disappears. The strength of the tone at the limen may then be calculated directly. The direct relation between strength of current and intensity of tone has been established for faint tones by Lord Rayleigh and Wien.

To determine the energy that is given off from the plate of the telephone one must obviously know the size of the vibrating surface, and the amplitude of vibration of each part. Wien¹ expresses the energy in the formula

$$A = \frac{C p o}{2 k} \Delta^2$$

A is the energy that passes through a square centimeter of surface in a second, c the rate of transmission of sound, p the tone of the plate, k the index of specific heat and Δ the relative pressure amplitude. Δ may be expressed in the equation

$$\Delta = 0.147 \frac{k}{c^2} \frac{(2 \pi N)^2 R^{2.00}}{\rho}$$

where π is the time of excitation, N the rate of the tone, R the radius of the vibrating plate, a the amplitude of vibration at the middle of the plate and ρ the distance between the ear and the plate. In practice one may measure the amplitude of vibration of the plate directly by attaching a fine glass rod

¹ Max Wien: Ueber die Empfindlichkeit des Menschl. Oohres etc. Pflüger's Arch., vol. 97, p. 1.

to the center of the plate with sealing wax and measuring the amplitude with a microscope with micrometer eye-piece. Measuring the current offers some difficulties if one uses an alternating current. Wien made use of a dynamometer but I have found it more satisfactory to use either the Edelmann Saiten-galvanometer or to use Pierce's method of a rectifier and ordinary galvanometer. The Edelmann instrument takes advantage of the tendency of a wire carrying a current to move in a magnetic field. It consists of a fine wire mounted in the field of a permanent magnet. The movement of the wire may be read with a micrometer eye-piece or in certain of the instruments may be photographed. The advantage of the instrument is that the moving part has a very small mass and the oscillations induced by an alternating current are as easily read as the deviation from a constant current. A disadvantage emphasized by Pierce is that the own tone of the wire favors one rate of vibration over others.

The rectifiers of Pierce depend upon the fact that certain crystals have a much higher resistance in one direction than in another to small currents. I had such a rectifier constructed of molybdenite and found that an ordinary D'Arsonval galvanometer was as sensitive to the alternating current as the smaller Edelmann instrument. The rectifier is much cheaper and easier to make or obtain than the other instrument, and fully as convenient to use. In use, then, it is only necessary to determine the amplitude of vibration of the telephone for one intensity of current that gives an amplitude that may be conveniently read, and that may also be easily measured by the instrument at hand, and then determine the current that gives a liminal tone. The liminal amplitude may then be computed and from that, the energy that is given off by the telephone and the amount that is expended upon the membrane of the ear.

We have also to consider the difficulty in determining the part of the tone that actually falls upon the ear, which gives as much trouble here as elsewhere. Wien obviated this by closing the telephone by a metal cap and connecting a tube with the opening that might be inserted in the ear. It is possi-

ble to assume that all of the energy affects the ear under these circumstances. The error in this assumption is probably less than on any assumption that one can make of the distribution of sound where the source and the ear are at a distance from each other.

One other difficulty that the telephone presents is that the breaking of the circuit is always followed by a marked click that is more noticeable than the continuous tone. The intensity of the click depends upon two factors. First the regular vibrations do not ordinarily release the plate altogether. Residual magnetism holds the diaphragm nearer the pole of the magnet than its position of rest during the entire period of its vibration. This may be observed through the microscope. Then, too, the forced vibration due to the current probably does not cause so great a vibration amplitude as the natural vibration of the plate that appears when the current is cut off. More important than either, however, is the fact that the natural pitch of the plate (usually about 900 vd.) corresponds to a rate of vibration to which the ear is more sensitive than the lower pitches ordinarily used in the laboratory experiments. Whatever the cause, the disturbance due to the click is very marked. Dr. Shepard, working in my laboratory, found that the click could be lessened to the point of not being noticed if two tones were superimposed upon the telephone. He used the commercial current of 60 cycles and a 250 VD fork. The physical basis for the effect is obscure, but the empirical effect is obvious.

Taken all in all the telephone is probably the most satisfactory instrument to use for sound experiments in the psychological laboratory. It can be made to give a constant sound that can be varied at will in pitch, in intensity and duration. It can be applied to the measurements of intensity in all fields, is invaluable in experiments in rhythm when combined with an interrupting apparatus and has a number of uses in subsidiary experiments. In addition, the apparatus required is less expensive than many of the pieces sold as instruments for the measurement of audition and the parts can be used in many other ways. One needs the telephone

itself, a galvanometer, a rectifier if one uses an alternating current, a tuning fork, otherwise, resistances that may be adjusted and a microscope for measuring the vibration of the plate. An induction coil that may be used as a step-down transformer is also convenient in saving resistance. All of these pieces are needed for other experiments and the cost of those that are not likely to be on hand will be less than fifty dollars.¹

In addition to these instruments that have been tested one might mention the instruments that have been used by Lord Rayleigh, by Webster, and by Toepler and Boltzman. Lord Rayleigh² used cans of ferrotype or tin made to vibrate by an electro-magnet. They were used to measure the relative sensitiveness of the ear to different tones and no attempt was made to determine the absolute intensity. The cans for different pitches were made similar in all of their parts. Intensity of the tones was determined by measuring the amplitude of vibration of the rims by a microscope. The magnets were within the cans and supported from the bottom. The alternating electric impulses were communicated by electric tuning forks. As the use of the instrument was restricted to measuring relative intensities extended description need not be given in this connection.

Very similar in principle is Webster's³ instrument. In brief it consisted of a resonator made to speak by vibrations of a plate of ferrotype that in turn was made to vibrate by an electric tuning fork. The ferrotype plate closes one end of the resonator and is connected with the tuning fork by a metal rod. The amplitude of vibration of the diaphragm may

¹ Seashore's Audiometer may be mentioned as an instrument that makes use of the telephone. For Wien's method it would need some means of measuring the vibration of the telephone plate and would also be more convenient if the resistance could be changed by smaller gradations. It would also need a tuning fork or some method of interrupting a current to give a pure tone. In many respects it is more convenient to use an ordinary resistance box and telephone with the forks and other auxiliary apparatus that are at hand in the laboratory.

² Lord Rayleigh: On the Relation of the Sensitiveness of the Ear to Pitch, Investigated by a New Method. *Phil. Mag.*, 14, 1907, p. 596.

³ A. G. Webster: On the Mechanical Efficiency of the Production of Sound. *Boltzmann Festschrift*, 1904, p. 866.

be read directly by a microscope. The instrument was used by Webster to measure the energy used and transmitted by sounding bodies, but would serve admirably as a reliable source of sound.

Toepler and Boltzmann measured the concentration of the sound in a closed tube by an interference method. It is more complicated than the methods described and need not be considered here.

In concluding this series of methods it may be interesting to compare the results so far obtained. The most striking result, as well as the one that shows most difference in the values, is the difference in sensitiveness to sounds of different pitches. The results of Toepler and Boltzmann,¹ Wead, Wien, Lord Rayleigh and Zwaardemaaker and Quix are given in parallel columns. The first column gives the rate of the tone in question and the column under the name

N.	T. AND B.	Wd.	Wn.	R.	Z. AND Q.
32					550
50			$4 \cdot 10^{-6}$		
64					37
96					$2,8 \cdot 10^{-1}$
100			$7 \cdot 10^{-9}$		
128					$2,7 \cdot 10^{-1}$
181	$3 \cdot 10^{-6}$				
192					$4,6 \cdot 10^{-1}$
200			$3 \cdot 10^{-11}$		
256		$83 \cdot 10^{-6}$		$8 \cdot 5 \cdot 10^{-9}$	$5,5 \cdot 10^{-2}$
384		$28 \cdot 10^{-7}$		$6 \cdot 10^{-9}$	$3,4 \cdot 10^{-2}$
400			$3 \cdot 10^{-14}$		
512					$1,97 \cdot 10^{-3}$
768		$31 \cdot 10^{-7}$			$2,5 \cdot 10^{-4}$
800			$7 \cdot 10^{-15}$		
1024		$11 \cdot 10^{-7}$			$2,7 \cdot 10^{-4}$
1536		$22 \cdot 10^{-8}$			
1600			$1 \cdot 10^{-16}$		
2048		$71 \cdot 10^{-7}$			
3200			$5 \cdot 10^{-16}$		
6400			$3 \cdot 10^{-15}$		
12800			$5 \cdot 10^{-14}$		

¹ Toepler and Boltzmann: *Pog. Ann.*, 141, 1870, p. 317.

the energy in ergs necessary to excite the ear.¹ It should be added that Zwaardemaaker and Quix used tuning forks in their determinations, and the values I have given are the results as corrected for certain errors by Wien in the second appendix to his article cited above.

The most striking feature of all of these experiments is the variation in the results. This variation, it will be noticed, is not only in the absolute values but also in the relative sensitivity to different pitches. The values do not seem to depend upon the method alone. It should be noted, too, that after Wien published his results that showed surprising differences in the limen for different pitches, Lord Rayleigh repeated his tests with the bells above mentioned and got results that showed that the limina for tones from 512 to 85 are related as follows, expressed with the limen for 512 as unity: 512, 1; 256, 1.6; 128, 3.2; 85, 6.4. He adds that he can see no reason for the disparity between his results and Wien's. Very obviously the results need to be gone over, but it seems to be no part of a report such as this to undertake the work. A new determination would have only the value that the methods and reputation of the worker give it and the older workers have apparently left nothing to be desired either in care or in the distinction of the men themselves.

Another method of varying the intensity of sound has been suggested and an instrument embodying it has been put upon the market. I refer to the audiometer of McCallie. I had hoped to have an opportunity of testing the instrument, but up to this time none has presented itself. In essentials it consists of some source of sound in a box and the intensity is varied by changing the size of the aperture that emits the sound. A priori the main objection to the plan is the fact that the intensity of sound will not vary directly with the area of the opening. Lord Rayleigh has shown that the sound will be very little affected by changing the width of the slit as compared with the length, where the slit is narrow. A change

¹The values in the columns Wn. and Z. and Q. assume that below 3500 VD two vibrations suffice to excite a tone, that seven are needed at 6400 and twenty at 12800, while in the others the figures indicate work done on the ear in a second.

in length from .5 inch to .28 inch compensated a change in the width of the slit from .004 to .020 inch. This proportion would certainly not hold for wider slits but the law for the relation between the area of opening and intensity of tone is not known and would probably be fairly complicated. The instrument also uses a noise rather than a pure tone, which in view of the results given above renders the measurement uncertain.¹ Rubber tubes are used to carry the tone to the ear as in the phonograph. This has the advantage of removing any uncertainty of the nature of the reflection from walls and the distribution of sound that constitutes an important source of error in the experiments that make use of free distribution through the air.

Of the sources of sound discussed here the telephone is the most convenient, and probably the results are as accurate as any. Next comes the damping method applied to the tuning fork, the others in a third group. It should be remarked that all of the methods make certain assumptions in the calculation of absolute values that may be in error and if in error, would serve to account for some at least of the divergence in results. Obviously a highly desirable advance in acoustics is the removal of these sources of error and the development of methods more suited to direct measurements.

Aside from this group of methods that depend upon the measurement of the sound emitted from the source, others are or should some day become possible that depend upon the measurement of the sound at the ear. The factors that make for errors in the calculation of the sound that passes through any given area at a distance from the source, even if the intensity of the sound at the source is accurately known, are many; some of them have been indicated above. The deflections and reflections induced by walls and obstructions are obvious. To these one must add the interference between the original and the reflected wave that tends to produce a dead region at a little distance from the wall. To these uncertainties of reflection one must add the fact demonstrated by Web-

¹ Professor Angell tells me that the sound has a tonal quality although it is described in the catalogue as a noise.

ster that the nature of the surface has a marked effect on the character of the transmission. He found that a sound could be heard three times as far over water as over a lawn. He argues that the grass acts as a black body in absorbing sound. The probability that other surfaces may show similar differences and the complete absence of knowledge on the point may well serve to enforce caution in calculating the amount of energy that passes through any area at a distance from the sounding body. One must look with a measure of skepticism upon all methods that have made such calculations and these include practically all the results of the men whose work has been cited above. Wien's results alone are derived with the ear placed so as to receive the entire energy of vibrations, and even his receptacle may permit certain losses.

These considerations make desirable some means of measuring the sound wave that actually falls upon the ear. This might be accomplished were it possible to obtain an instrument delicate enough to measure the force exerted or the work done by the vibrations. Two instruments approach this degree of sensibility, the telephone of Pierce¹ and the receiving instrument of Webster. The Pierce instrument measures the electric current induced in the telephone circuit by the displacement of the plate of the telephone by the sound wave. The apparatus for measuring the current was described above. This with Pierce's adjustment was barely adequate, but it is no doubt possible with practice to get a more sensitive adjustment. Webster's instrument has not been described in print, and I have never seen it, but I am told that in essentials it consists of a glass plate that is made to vibrate by the sound wave and the slight movements of the plate are measured by an interference method. How delicate it may be I do not know. It is highly desirable that one or the other of these methods should be perfected to the point that will permit them to check the results obtained by other methods. Even if one could measure the energy where the ear is to be, when the ear replaces the measuring instrument the conditions would be changed in some

¹ G. W. Pierce: A Simple Method of Measuring the Intensity of Sound. *Proc. Am. Acad.*, vol. 43, p. 377.

small degree, the head and body would reflect sound differently from the plate or telephone. This error would undoubtedly be much less than those that are made in assuming a law for the distribution of sound in an open space, but still would hardly be negligible. The method when developed will probably be more accurate than any that depends upon the energy developed by a source of sound, and if the promise is not altogether realized it will at least provide an invaluable check upon the other methods.

RECOMMENDATIONS.

We may close with a series of specific suggestions based upon the tests and the examination of the literature.

1. One should always use a tone as pure as possible as the stimulus. A noise is nothing other than a tone of unknown pitch or a complex of tones of unknown pitch. So much of the sensitiveness of the ear depends upon the pitch that if that be unknown or neglected the result is uncertain. The preference of most psychologists for the use of noise seems entirely without rational support.

2. All methods that depend upon the impact of falling bodies give uncertain results. (a) The sound is always a noise. (b) The pitch of the noise can not be kept constant. (c) No known relation exists between the energy developed and the amount transformed into noise. While the relative intensities vary with the height of fall, the method gives no means of obtaining absolute measurements.

3. The most accurate and convenient instrument for absolute measurements is a telephone actuated by a tuning fork or alternating current. Some method of measuring the strength of current and a micrometer for measuring the amplitude of vibration of the plate is also required.

4. Slightly less expensive and simpler is the equipment for using the tuning-fork method. A fork with stable mounting, and a micrometer, or a fork whose rate of damping has been determined is all that is required. The results are probably less accurate than those obtained by the telephone, but that is not altogether assured.

THE MEASUREMENT OF PITCH DISCRIMINATION: A PRELIMINARY REPORT.

BY C. E. SEASHORE.

This report was called for by the Association in the belief that the systematic criticism and trial of current methods and means, and the statement of the essential implications, would economize effort for future workers and further the prospects of practical applications of the test. The measurement may be made by so many different kinds of apparatus of various degrees of worth, by so many methods of procedure more or less adequate, under so many hypotheses more or less specious, for so many purposes more or less legitimate, that the situation is very complicated.

This preliminary report is limited to a bare outline of the nature of the problem with tentative, positive recommendations in regard to the procedure.

The outline features here presented have served and should continue to serve as an aid in concentrating research on the most essential problems. Among the problems already taken up in the Iowa Laboratory are the following: the physical constants and variables in the tuning fork, the string, the reed, the pipe, the tone variator (bottle), and the siren; the adaptation of different types of instruments to different needs, and different psychophysics methods; the evaluation of one method in terms of another, *e.g.*, the method of the "average of the middle third" in terms of the method of right and wrong cases; the validity, *i.e.*, the degree of certainty or constancy in the measurement; the effect of intensity, duration, length of interval, order of trial, tone quality (timbre) and prolonged trial (fatigue), on the discrimination; the relation of consciousness of difference to actual difference (very surprising); the lower limit of tonality; the relative

discriminative sensibility for pitch within the tonal range; the relations of pitch discrimination to other musical capacities, *e.g.*, the sense of rhythm, ability in singing and playing, the perception of dissonance and the appreciation of music; also its relation to musical education and musical environment; the possibility of improving the capacity with practice; the establishment of norms; the relation of the cognitive to the physiological threshold; variation with age, sex, and general intelligence and special intelligence; and pedagogical applications. This report is based essentially upon the result obtained up to date in the investigations just named; general acknowledgment to these workers is here given. Volume VI of the *University of Iowa Studies in Psychology* will be devoted to the publication of some of these investigations.

These are large problems. A few have been solved in part, but most of them require a long time. This announcement of problems should in no sense be regarded as preëmpting the field. What we need is elimination of useless effort, coöperation, and stimulation of interest on the part of capable investigators. The committee therefore most cordially urges those who are engaged in, or can undertake, research in this direction to correspond with the chairman of this sub-committee in order that the work may be correlated. There will be no restrictions or interference, but it may enable us to avoid known snags and duplication of effort, and should ensure effective collaboration.

The sub-committee presents this preliminary report as an aid to the better formulation of work in the subject. The extended reports of research on these problems will be published whenever and wherever convenient, and it is to be hoped that, after a few years, the results may be summarized into a more comprehensive and authoritative report—a sequel and, in part, a result of this preliminary report.

There are many and varied demands for the measurement of pitch discrimination: *e.g.*, in the decision about whether or not to start a child in a technical musical education; in the classification of pupils for the purpose of class instruction; in anthropological measurements; in psychological measure-

ment as a means of determining the specific laws of pitch hearing and production, as well as in the working out of the general laws of mental life such as in attention, fatigue, association, suggestion, imagery, memory, automatism, and affective tone; and in the countless applications of these psychological and psychophysical facts to a theory of esthetics, with reference to the application and the expression of music as well as to the pedagogy of musical instruction.

I. GENERAL ACCOUNT OF APPARATUS.

Pitch discrimination is usually measured by determining the least perceptible difference in the pitch of two successive tones. Without actual measurement, the pitch discrimination is often estimated quite accurately, *e.g.*, by observing the accuracy in singing and in the tuning and playing of certain instruments, as well as in the ability to analyze musical clangs. The measurement may be made by means of tuning forks, stringed instruments, reeds, pipes, sirens, etc. The relative merits of these instruments and the means for using them should be set forth in a later report.

The measurement of pitch discrimination has a short history. While musicians have always wrestled with the problem in a general way, the significance of accurate measurement has not been comprehended until in recent years. It may truly be said that the discovery of the real significance of the measurement of pitch discrimination came gradually with the discovery of the sources of error which enter into the test as worked out in the psychological laboratories.

Much time and energy have been wasted and false returns have been announced from experiments with unreliable apparatus. Instruments for one kind of work have been used for another. There has been no satisfactory coöperation in the designing of instruments and in the securing of critical tests before putting the apparatus on the market. The makers have not had the right encouragement or discouragement.

A. *Factors in Apparatus.*

The principal variables, the control of which determines the reliability of the apparatus, are: (1) form of the vibrating body—fork, reed, string, etc., (a) with reference to the selection from these fundamental types of vibrating bodies, and (b) with reference to the favorable construction of each within its own style; (2) the quality of material and workmanship; (3) accuracy of tuning; (4) mode of energizing; (5) mode of reinforcement and purifying; and (6) constancy of conditions. With these requirements in view, one must take into consideration, among other factors, the following.

1. *Reliability.* The measurement is made in terms of double, or complete, vibrations (vd.)¹ As a most serviceable standard of reliability in the apparatus and accuracy in tuning, we recommend $\pm .05$ vd. for increments from .5 to 2 vd.; $\pm .1$ vd. for increments from 3 to 5 vd.; and $\pm .2$ vd. for increments larger than 5 vd. This concerns the reliability of the original tuning, or the ability to set or tune, as well as the reliability of the apparatus, *i.e.*, freedom from progressive or constant errors.

We must distinguish between rough and accurate tests. One is as legitimate as the other in its place. We must also distinguish in the latter between tests of persons who have a high threshold and those who have a very fine, low threshold. This discussion of apparatus is concerned with accurate tests in which small sources of error would be disturbing.

2. *Availability.* In addition to being reliable, the apparatus must also prove available, *e.g.*, obtainable as regards cost, adapted to the necessary method which serves a specific purpose, and adequate to the present needs as regards time saving, accuracy, etc.

The various types of tuning forks, strings, reeds, bottles,

¹ Much confusion in terminology and in the interpretation of records has resulted from the fact that in some scientific work, notably the French, the single vibration (vs.) has been used as a unit, and both single and double are spoken of as vibrations. It is therefore desirable to use the differentiating abbreviation vd. in this country where the double vibration is the unit.

and sirens, each have some legitimate place. Thus the tone variator is our only available instrument for the measurement of gradual change in pitch; a well built monochord is very convenient for rough tests in a class; and a pair of electrically energized tuning forks with selective resonators is very satisfactory for the testing of an individual or group of individuals whose thresholds are known to correspond to the tuning of the forks, and when a sufficient number of trials can be made to justify the use of the method of right and wrong cases. But the first two of these are not reliable for accurate work and the last is not available for the tests most used.

We therefore recommend that, before the records are made, the adaptation of the instrument to the measurement in question be carefully considered.

3. *The Tonal Register.* In many cases it is necessary to make measurements at different levels of pitch. This may fix for us in part the choice of apparatus. Some instruments are adapted for very limited range and others for an extensive range of pitch. In statistical and other comparative tests, it is desirable that we should accumulate data with reference to some standard pitch that is decidedly favorable and can command general adoption.

In view of the available apparatus, the agreeableness of the tone, the fact that it represents the middle of the register of tones most used, that it is in relatively the most sensitive register, and that it is the international standard of pitch, we recommend that standard tests be taken at a' , 435 vd.

For the sake of securing uniformity, although the matter is quite arbitrary, we also recommend that the series of increments upon that standard be chosen above rather than below standard.

4. *Increments.* In a discrimination test of this sort, the change of pitch must of course be an absolute step, not a gradual change. The range of increase should be so chosen as to cover all cases in normal groups. .5 vd. for the smallest increment and 30 vd. for the largest increment at the standard of 435 vd. is adequate for most purposes.

Within this range the arbitrary steps chosen in extensive

experiment seems to require (1) as small difference as would have any significance for a group test in consideration of economy and trustworthiness, and (2) intervals of relatively equal psychophysical significance.

The increments should be chosen in some geometrical ratio, but the strict geometrical ratio would necessitate superfluous steps—too small increments at either end of the series. Furthermore, there is a real advantage in maintaining the increments in whole units without using fractions of a vibration. We therefore recommend as economical and serviceable, the following series of increments above the standard a' , 435 vd.: .5, 1, 2, 3, 5, 8, 12, 17, 23, and 30 vd. It will be observed that, except for the first two steps, this is an arithmetical progression of the second order.

It is probably just as valuable for us to know whether the threshold is .5 vd. or 1 vd. for the fine ear as it is to know whether it is 5 vd. or 8 vd. for a medium ear, or 23 vd. or 30 vd. for a very inferior ear. This equality is of course only an empirical approximation, for convenience and economy.

5. *Sounding.* In view of the fact that timbre, intensity, duration, direction of sources, etc., must be approximately constant; that pitch must be accurate and the tone constant during the time it is sounded; and that there is grave danger of identifying the tones; the mode of energizing the vibrating medium becomes a delicate matter. Many forms of apparatus are ruled out because they have some obstacle to effective energizing.

We therefore recommend that only such apparatus be used as will enable the experimenter to produce in rapid succession two tones which are practically alike in all respects except pitch.

6. *Timbre.* The richer the tones the more opportunity there is for discrimination to fasten more or less unconsciously upon some difference in the character of the tone as a means of identification. Thus, two tuning forks fixed upon wooden resonators invariably assume individual peculiarities by which they may be identified; two strings bowed in succession are open to the same objection. Even aside from this error

of identification, impure tones are probably more readily distinguished than pure, unless the impurity is of such nature as to be distracting, as in a badly bowed string. This relatively greater ease in discrimination for rich or impure tones is probably due to aid from other characters than pitch, and therefore illegitimate. There are two ways of dealing with it: one is to use a selective resonator, *e.g.*, A Koenig or a Helmholtz resonator, or a water resonator; the other is to transmit the vibration through a common medium, *e.g.*, by telephone. The telephone acts remarkably well as a selecting instrument, when used as in ordinary telephoning, and the timbre of the tone produced in the single receiver becomes quite uniform, when the sound comes from approximately uniform sources, but the tone is not as pure as when heard from the resonator directly.

In view of these facts we recommend that the test be made with the purest tone available. As far as we know now, this is best produced by an unmounted tuning fork reinforced and purified by means of a selective resonator.

7. *Resonance.* The sound is most favorable for pitch discrimination when it is just loud enough to be clearly heard without effort. Loudness depends chiefly upon the resonance of the mounting and the surroundings of the vibrating body. It is largely in the reinforcement that the tone gets its individual character; and the increased resonance usually results in increased richness of tone, which is a source of most serious disturbance in this test.

If a single individual is to be tested, a tuning fork, for example, may be held close to the ear with good result without a resonator. For tones lower than 75 vd. the fork thus presented should have hard rubber disks, about 8 cm. in diameter, mounted on the outside of each prong. Such disks strengthen the tone satisfactorily. But, for the ordinary work, with tones near the middle of the register we recommend, as in 6 above, the use of the selective resonators with good tuning forks unmounted.

8. *Danger of Identification.* One of the most insinuating and persistent obstacles—one most frequently overlooked—

is the possibility of identifying one or both of a pair of tones by other means than pitch difference pure and simple. The commonest means of identification are timbre (purity) loudness, characteristic difference in facility of handling, and location of the sounding body. The situation is doubly complicated by the fact that if the observer merely imagines, or has an illusion to the effect, that he can identify a tone by one of these accessories, this is just as fatal to the test as if he actually did identify. And it is further aggravated by the fact that the more effort we make to secure the desired uniformity, *e.g.*, absolute equality in intensity of tone, the more of a temptation we set the observer for pouncing upon this as a means of identification. And, worst of all, the identification may work itself out subconsciously, and indeed it ordinarily does, without the observer being aware that the accessory factors play any rôle, in the estimation of pitch.

For these reasons we are frequently forced to discard apparatus which is reliable in itself and may have other advantages. Our general solution is to get as much uniformity as possible without making it constant, *e.g.*, the intensity of a tone is kept approximately uniform by placing the successive forks at approximately the same point before the resonator. But there is a constant slight variation which cannot be predicted or verified. The same applies to timbre, duration, mode of presentation, etc. The observer should understand that although these factors are kept fairly uniform, they are not constant and that he can find no means of identification in them because they vary fortuitously within small limits. The apparatus must therefore be constructed and manipulated so that this condition can be carried out, *i.e.*, the accessory variation shall be the very minimum, but the observer shall have the positive instruction that the variations are not peculiar to any one tone.¹

¹An illustration of this difficulty is found, *e.g.*, in using an automatic hammer to strike the string or fork. The hammer produces more uniform intensity than we can produce by a stroke of the hammer in the hand; but after a few trials the observer will detect some constant peculiarity of that stroke, and from that moment the test is vitiated. And even if the observer does not detect any difference the chances are that he will be influenced subconsciously by automatism in the direction of identifying tones.

We therefore urge that no test be undertaken, or record accepted, unless such precaution has been taken with reference to apparatus, method, and efficiency in manipulation as to guarantee that this danger of identifying one of two tones falsely has been ruled out.

B. Apparatus Rejected.

As stated above, a monograph on the availability and the reliability of the instruments which have been used in this test is in preparation and will be published with measurements in detail. Each instrument is tested in several respects by means of the tonoscope, which makes ready and accurate measurement of all instruments by the same means to the accuracy of $\pm .02$ vd.¹ The measurements have been carried far enough to lead us to reject, for our present purpose, the reed, the piston pipe, the bottle (tone variator), the siren, and the string. All of these are available for rough work and each has some superior feature which may justify it for some special purpose. We must, however, await the publication of the details of the examination before assigning each to its place. In the meantime any one who is engaged, or proposes to engage in research with any of these instruments, may obtain available information by corresponding with the writer.

For our present purpose, all these instruments are rejected (1) because they do not give a sufficiently pure tone, and (2) because, under the conditions ordinarily employed, they are not sufficiently accurate.

We urge that no serious research be undertaken in pitch discrimination unless the experimenter either finds authoritative standardization of his instrument on record or himself undertakes careful tests on the instrument before using it. This is a commonplace rule in psychology but it is as frequently violated as it is commonplace.

¹ This is in rapid reading. For finer adjustment than $\pm .02$ vd. it is more convenient to use the beat method.

C. Apparatus Recommended.

In view of the above and other considerations we recommend as the best available and most serviceable apparatus, a set of twelve unmounted tuning forks, a Koenig adjustable resonator, and a simple sounding rod.

The forks best suited for the purpose are of the grade marked 22807 in the catalogue of Max Kohl, Chemnitz, i, S., Germany, and will be known as the "standard pitch discrimination set." The set includes two standard and ten increment forks. They are tuned to the accuracy recommended in Sect. 1 above, for the increments recommended in Sect. 4 above at the standard recommended in Sect. 3 above. They may be imported duty free for about \$41.00. The same set of forks untuned (all tuned to 435 vd.) may be imported duty free for about \$34.00.¹

A cheaper and smaller, yet serviceable, grade of forks is advertised as No. 1730 in the catalogue of the C. H. Stoelting Co., Chicago, and may be obtained tuned approximately the same as the above set for about \$18.00. The same forks may be obtained untuned for about \$12.00.

It is essential that the forks shall be fairly heavy, and of uniform size and shape (except for tuning) and of good material and workmanship. There are many styles and grades of forks on the market which would answer the purpose.

The resonator is a stock piece of physical apparatus, marked in the catalogues "Koenig adjustable resonator, mi^a-la" and may be imported duty free for about \$6.50.²

For a sounder, use 1 inch square rod covered by very heavy soft rubber tube and fastened in a horizontal position firmly to some heavy metal support, so that the forks can strike on one edge of the rod.

¹ These forks are 14.5 cm. long, with prongs 9.5 mm. wide and 5 mm. thick, and perfectly uniform in shape.

² In lieu of this a serviceable resonator may be improvised by taking a graduate or a plain glass tube about ten inches long and $1\frac{1}{4}$ inches in diameter and pouring water into it until it is tuned to the right pitch. Or, a brass tube may be cut to the proper length and one end corked. Such devices give a good quality of tone but it is difficult to get the tone loud enough for a large room.

An opaque screen should be provided to prevent the observer from seeing any movements of the experimenter.

D. Directions for Tuning.

Since the experimenter must at all times be ready to test and, if necessary, correct the tuning of the forks; and since the difference in price is considerable, it is generally a good plan to buy the untuned forks.

Raise the pitch of a fork for each of the ten increments by filing equally, symmetrically and squarely on the ends of the two prongs—not on the side of the prong as is usually done. Time by counting beats between the filed fork and the standard. For final tests count the beats for as long a period as they can be heard. As one cannot count more than three beats per second with certainty the 5vd. fork may be tuned from the 3vd., the 8 vd. from the 5vd., etc., Avoid the sources of error mentioned above and following, *e.g.*, temperature, force of the blow, the place of striking, etc. In order to reduce the effect of sympathetic vibrations from one fork upon the others hold the forks before the resonator as far apart as may be compatible with audibility of the beats. Test to the accuracy recommended in Section I above.

II. PRECAUTIONS TO BE OBSERVED IN THE TESTS.

A. With Reference to Apparatus.

1. *Temperature.* Use each of the two standards alternately for short periods, and hold all forks with fingers near the end of the stem of the fork.

The pitch of a fork falls by .00011 vd. for each degree centigrade of rise in temperature, according to Lord Rayleigh. Larger differences than this are quoted by experimenters, but it is probable that the effect of the temperature varies with the pitch, size, form, etc., of the fork; perhaps also differently with means and extremes of temperature. A 435 vd. fork just taken from boiling water is more than 4 vd. lower in pitch than the same fork just taken from ice. This error is,

however, practically eliminated: (1) by keeping all the forks in the same atmosphere, and (2) by using the two standards alternately to reduce the cumulative effect of heat from striking and from contact with the hands, and (3) by holding all forks near the end.

2. *Position of the Blow.* Hold the fork so that it strikes the sounder at the middle of the prong and squarely on the side.

A blow near the middle of the prong starts the prong to vibrate most favorably—faster and more regularly than when struck either at the tip or at the shoulder. An extensive series of tests with one fork gives the result: middle, 434.38 vd.; tip 434.22 vd. The blow at the middle of the prong gives the best quality of tone.

Ideally, both prongs should be snapped so as to start synchronously in true phase; but this is impracticable, therefore we adopt the plan of striking only one prong, but under uniform conditions. The vibration frequency of the fork is irregular until the two prongs vibrate symmetrically.

3. *Force of the blow.* Strike the sounder uniformly with as light a blow as will produce a distinctly audible tone through the resonator.

A light stroke produces the smoothest tone, a tone that is most favorable for true pitch discrimination, and a tone that remains uniform for relatively the longest period; a strong blow produces a harsh and changing tone and injures the forks. The blow should be a light tap with free rebound, in order that the initial impetus given the prong may not be modified by continuous pressure or repeated contact.

4. *The Sounder.* Make sure that the sounder is soft, well rounded and firm, with a minimum of resonance.

A springy sounder prevents the proper rebound of the fork. Any harshness or other unpleasantness in the thud is likely to serve as a distraction, coming as it does just at the moment attention is to be focussed.

5. *Adaptation Period.* Allow at least one second after striking before presenting the fork before the resonator.

Even under favorable circumstances, the first sound of the fork is rough, and the vibration frequency varies until

the two prongs vibrate symmetrically. This makes it inadvisable to sound a fork resting on a resonator, by striking or blowing.

6. *The Resonator.* Suspend the resonator so that it can vibrate freely; present the fork quickly and uniformly by bringing it straight toward the mouth (not from the side) of the resonator, so that the upper end of the side of one prong covers the opening; withdraw it in the same line.

If the resonator were laid on a solid, e.g., a table, that body would add a disturbing resonance. Presenting the fork from the side gives a rough edge to the resonator tone and may give a clue to identification of forks. A quick and firm movement of the hand insures a clear cut beginning and end of the tone. Ordinarily the fork should be held as close to the resonator as it can be without any danger of touching; yet it is possible to hold it so close that a recognizable change in quality of the tone is produced.

The wooden resonator is discarded because it has a very rich and variable accessory resonance.

7. *Testing the Forks.* Test the forks frequently and keep record of the changes made in a given set of forks. Small forks like these may change vibration frequency by handling. Hard and irregular striking is probably the greatest source of the derangement of forks. A tuning fork should be handled like a tuned string,—with care. This is especially true of the lower forks which represent the small increments. Wherever accurate measurements are made, the lower forks should be tested frequently. In testing, all the variables must be eliminated. No fork should be filed until it is perfectly clear that it is faulty, and then only after its relations to the rest of the series has been determined.

B. With Reference to Procedure.

1. *Intensity.* Make the two tones equally loud—just so loud that they can be heard distinctly without effort.

One of the most serious obstacles we encounter in this test is the effect of intensity or loudness upon pitch. An immature

observer is likely to identify high and strong to some extent. A more developed or trained observer may reverse the association and show a tendency to judge the weak tone high. For this reason, various contrivances have been designed to secure uniformity in the intensity of tones. We here recommend that the experimenter shall practice, and simply trust his own ear and hand for approximate uniformity. If a fork is sounded, either too weak or too strong, the trial should be repeated. This method is adopted because it prevents the observer from attaching importance to heard intensity differences. When mechanical contrivances are used to produce uniformity in intensity, the observer either hears or fancies he hears a difference which is consciously or unconsciously taken as a cue for the identification of the fork.

Extensive experiments show (1) that both trained and untrained observers may be influenced by intensity in their pitch judgment; (2) that although there is a tendency among the untrained, especially the ignorant, to judge the loud tone the higher, it may work either way; (3) that the same individual may show one tendency at one time and the reverse at another; (4) that for trained observers the two tendencies are about equal; and (5) that the tendency is more serious for large than for small intensity differences. Introspection shows that this confusion rests largely on motor tendencies or motor images. We associate high and strong with strain—the reversal can in some cases be traced to a correction, conscious or unconscious, based on knowledge of this danger.

Experiments show that the just perfectly clearly perceptible tone is most favorable for accurate results. It is ordinarily purer than a stronger tone and favors concentration. Experimenters must guard against a very common tendency, usually unconscious, to facilitate the discrimination by making the tones loud; and untrained observers usually desire (unwisely) a loud tone.

2. *Duration.* Sound each tone about 1 second. The most favorable duration of tone is about $\frac{1}{2}$ second. This cannot be obtained in a simple method of manipulation; and the variation with duration from $\frac{1}{2}$ sec. to 1 sec. is not large.

The ratios found by experiment are: $\frac{1}{4}$ sec., 73 per cent; $\frac{3}{8}$ sec., 82 per cent; $\frac{1}{2}$ sec., 84 per cent; and 1 sec., 82 per cent. We may therefore content ourselves with as short a tone as can be produced consistently and economically with clearness and uniformity. In a practiced experimenter it approaches one second, and this should be kept constant. Here again both the experimenter and the observer have a tendency to crave a longer duration.

For most accurate work, in which duration is a factor to be varied, or is otherwise important, mount a revolving disk in front of the mouth of the resonator. Control the speed of the disk and cut slits in appropriate sectors to regulate the duration of tone and the time interval between the two tones. In this case it is necessary to have one resonator for each fork. The fork may be held by the hand in the usual way. This also has the advantage of forcing the trials in rapid succession and that favors the discrimination.

3. *Time Interval.* Hold one fork in each hand and strike them in rapid succession; then present them to the resonators so as to make the time interval between the two tones about one second.

Discrimination for successive stimuli always involves the memory element. The curve of tonal memory, as is well known, is a parabolic curve showing that the accuracy of memory falls off very rapidly, immediately after the first second of interval. But for intervals from $\frac{1}{16}$ sec. to 1 sec. there is practically no difference, as is shown by the following ratios: $\frac{1}{16}$ sec., 80 per cent; $\frac{1}{8}$ sec., 75 per cent; $\frac{1}{4}$ sec., 81 per cent; $\frac{1}{2}$ sec., 78 per cent; and 1 sec., 75 per cent. It is therefore important that the time interval be kept fairly constant and that it should not exceed 1 sec. The most favorable interval may depend in part on temperament and training of the observer.

4. *Order.* Determine the order of presentation of the higher and lower forks by chance; use keys prepared beforehand by tossing a coin. Modify the "chance order" of the key so that there shall be no more than three trials of the same order in succession.

If two forks of the same pitch are sounded in rapid succession some have a tendency to call the second higher, others lower. This error is eliminated by alternating the order of the standard and the compared forks in this chance order.

It is important that this order should be determined objectively. On account of the community of ideas and a natural tendency to anticipate, arbitrary choice of order by the experimenter would lead to guessing "what he will do," and expectation, which would influence the judgments very seriously.

5. *Uniformity.* Avoid identifiable uniformity. Many devices are available for securing uniformity automatically. Thus, a pair of electrically driven forks, with necessary contact may be used, as the resonator does not speak to the accessory sound. Or, if the accessory sounds are considered disturbing, the forks at the resonator may be driven "tandem" by another pair of forks in a distant room. The electrically mounted forks may be held, unmounted, in the hand. Automatic hammers may be so mounted on the forks as always to strike the fork in the same place and manner and with the same degree of force. The forks may be so mounted that they swing automatically into position before the resonator. A resonator may be mounted permanently before each of the automatically struck or electrically driven forks. The duration and the interval may be regulated by a rotating disk or pendulum.

These and many other devices have been tried with the general result that all except the last (the regulation of duration and time interval) have been rejected. It required more skill to operate one of these devices *safely* than to follow the simple plan recommended. The ever present and practically insurmountable danger is that of identification of a tone by some permanent character other than pitch.

6. *Fatigue.* Adapt the length of the test to the endurance of those tested.

A test of this sort is novel and requires continuous conscious effort and is, therefore, surprisingly "fatiguing." The ear itself does not fatigue in the same sense as the eye fatigues,

but the test requires an unusual concentration of attention, and therefore causes a general exhaustion.

Extensive experiments on this specific problem show: (1) that, even through the most severe continuous and uninterrupted strain for two hours, a normal, reliable, adult observer (a) feels no fatigue or strain in the ear, and (b) shows no progressive loss of keenness of discrimination; (2) that there are marked periodic fluctuations in capacity which usually correspond to felt distraction, ennui, or lack of effort; (3) that what is generally known as fatigue in this experiment is primarily an unjustifiable feeling of restlessness or blaséness coming from the habit of change and, secondarily, discomfort from position; (4) that, in normal observers, the discrimination tends toward automatism and becomes easier so far as effort is concerned during uninterrupted progressive adaptation; (5) that in some persons the strain brings on a headache; (6) that the test is far more straining on the person who is not familiar with it than one who is; (7) that after extreme exertion there sometimes follows a reaction in the form of a feeling of exhaustion, especially if the observer has worked in a continuous "heat" unconscious of his bodily self; and (8) that in such an endurance test good results are obtainable with ease in proportion to the absence of distraction. In short the strain of the test is "wearing" on the observer and, although the progressive adaptation is favorable to the ease and accuracy, it is difficult to secure continued application for long periods.

Therefore with primary children the test should not be carried beyond ten or fifteen minutes of continuous testing at a time. With grade and high school children, a half-hour test is most favorable, and even with adults two half-hour tests are more favorable than a one hour test.

7. *Attention.* Secure the most favorable form of attention, the secondary passive; and favor economic distribution of attention by proceeding rapidly and regularly.

It is all important that the observer should trust the primary impressions, and that systematic warning should be given in such a way as to sustain the most favorable form of an

attention wave. The variation of a fraction of a second from the time at which a stimulus is legitimately expected is likely to modify the record. In a test of this sort it is always assumed that the judgment is made under the conditions of maximum efficiency and attention.

8. *The Charge.* Impress the individuals or groups to be tested with the seriousness of the test and their accountability for every judgment.

There is always a certain amount of lethargy in a group of individuals, and the group feeling tends to lessen individual responsibility. It is, therefore, necessary that a very specific charge shall be made to the group at the beginning of the test. Call attention to individual responsibility, the personal nature of the record, the significance of attention, the demand of absolute integrity etc.—and do it with a vim. Be specific and firm. A charge of that sort may lower the average record for the group very materially. We must always assume that an effective charge has been made.

9. *The Judgment.* Always require the form of judgment which is characteristic of the method of right and wrong cases, *i.e.*, the observer is limited to two judgments (higher or lower); or, if the third judgment (equal) is allowed, the records of that equal judgment must be distributed in accordance with the rules of the method of right and wrong cases.

This rule is made mandatory by the fact that (1) the illusion of difference is very insistent and (2) that there is no close correlation between different degrees of certainty and actual difference near the threshold. We know rightly with a fair degree of certainty whether or not we hear a sound, or whether it is strong or weak, but not so with pitch of sound. We have distinct convictions that we hear difference in pitch often when there is no difference and when the difference is below the threshold.

III. CLASSIFICATION AND ORDER OF TESTS.

All tests may be divided into (1) preliminary and (2) final. Each of these may be made as (1) group or (2) individual

tests. The preliminary group test is heterogeneous, the final homogenous. The heterogeneous group test is one in which a group of individuals, *e.g.*, a grade, a class, a type, or a community, is tested *en bloc* for the purpose of classifying the individuals in the group as a preliminary to the final test, or for rough and rapid permanent classification. The homogenous group is one in which the individuals have been grouped together on the basis of approximate equality in pitch discrimination, as determined by some preliminary test. For the final test we may employ a well recognized method; for the latter it is necessary to use an improvised method. Both are described in the following section.

The extent and order of the test should be determined in view of the purpose to be served, time available, means at hand, etc. For thorough examination of large numbers we recommend the following order of procedure:

1. A one-hour test of the heterogeneous group¹.
2. A one-hour "final" test of homogeneous divisions of the group, based upon the classification obtained in 1.
3. Homogeneous group tests with full introspection, *ad libitum*.
4. Individual interview, check trials, and extended "final" individual test.

IV. DIRECTIONS FOR TESTS

A. *Preliminary.* When the first, or only test is to be made on a heterogeneous group, set aside one hour of time for the actual test. Supply each individual with a cross ruled paper, designating the vertical columns by numbers from 1 to 20, and the horizontal columns by the letters, A to J. Call the increments A, B, C, D, etc., beginning with the largest. Thus, A denotes a difference of 30 vd.; B, 23 vd., C, 17 vd.,

¹ The limit to the numbers of persons that may be tested at one time depends upon a number of considerations such as discipline under control, the loudness of the tone, the homogeneity of interest in the group, the acoustic qualities of the room, the skill of the experimenter, etc. Under favorable circumstances, one can readily handle a group of 200 to 300 persons at one time for a preliminary test.

etc. Illustrate to the group how the forks are sounded and what is meant by pitch.

Then give instructions to the following effect: I will sound two tones in quick succession. You are required to record whether the second is higher or lower than the first. There will always be a difference in the pitch, but if you can not hear it, you must guess. If the second tone is higher, record H; if it is lower, record L.

Give a few preliminary trials in which all are required to speak L or H instead of writing it. Follow this by some illustration of differences in intensity until all distinguish clearly between intensity and pitch. Urge them to trust the first impression, and warn them not to despair when they feel in doubt. Suggest that they keep their eyes closed when listening. Explain that before each trial you will give them warning by designating the square in which they are to record. Thus: column 1, A; B; C; etc., and in the same manner in succeeding columns.

In determining the order of the sounds, follow a key made out beforehand, as directed above, to determine whether the standard shall be sounded first or second. This operation of the law of chance should be explained to prevent guessing or probabilities.¹

All being prepared, give without interruption one set of trials, A to J, and then allow a one-half minute rest. Take as many sets of trials as the time permits. A skillful experimenter can take about eighteen sets in an hour. If the record is not to be used in any important way, read off the key slowly, after completing five sets, and require each individual to check errors by drawing a bold line through the letter which is wrong. The experimenter must adapt methods to conditions. For example, each observer may be allowed to check his own record or they may be required to exchange. The experimenter must also use his judgment in deciding whether or not it is necessary to counter-check the records.

¹ It should be explained in particular that if the coin has fallen head up, *e.g.*, two times in succession the probability is still exactly equal for head and tail.

Request each observer to make note of anything that may aid in the interpretation of the record and its significance. For certain purposes it is desirable to have systematic information about each individual, as regards musical training, attainments, environment, appreciation, etc. The following questionnaire has been found serviceable.

Name Age Date Hour Class

Please give as specific and detailed information as possible in regard to your:

- I. **MUSICAL TRAINING**
 1. In public schools
 2. Private vocal lessons (when, where, how long, etc.)
 3. Private instrumental lessons (when, where, how long, etc.)
- II. **MUSICAL ENVIRONMENT**
 1. Instruments in your home, and their use
 2. Musical encouragement at home (trained voices in family)
 3. Opportunities for hearing music of any sort (specific)
- III. **MUSICAL EXPRESSION**
 1. Favorite selections you can sing (by ear? by note?)
 2. Favorite selections you can play (by ear? by note? instruments?)
 3. Singing or playing in public (parts, occasions, etc.)
- IV. **ENJOYMENT OF MUSIC (WHAT ACTUALLY APPEALS TO YOU)**
 1. Vocal (solo, quartette, chorus, opera, popular songs, classics, religious, secular)
 2. Instrumental (solo, symphony, band, etc.)
 3. Characteristic effects of music (mental, physical)

We begin with a large step because that gives the observer a clear grasp of the nature of the difference and starts him with confidence in his judgment. We take a set at a time (vertical column) instead of giving successive trials on the one step (horizontal column), because the short set makes a favorable distribution of strained attention. We take the whole set of ten steps each time primarily because we are dealing with a heterogeneous group of observers, and secondarily in order to approach the threshold under uniform conditions. Twenty sets really amount to twenty trials, each

set being a trial to see how far down on the scale of increments the observer can hear correctly. Given the range of increments, and the allotted time, the task is to secure as many trials as may be needed for a fairly reliable record within this minimum period of time.

The computation of the records must be left to the experimenter. Run through the checked records and place a tally mark for each set in the horizontal column above the one *in which the first error occurred* in a set. The completed tally will then show the distribution of the thresholds for sets in terms of the smallest increment for and above which all judgments are correct.

Compute the *medians*. The formula for the median may be found in Titchener's *Instructors' Manual*, Quantitative, pp. 8, 9 and 361.

Supplement this numerical record by notes on interhal evidence in the record, such as modes, progressive changes, confusion, mistakes in record, etc., and, when fully justified, enter a numerical estimate of a correction. The record thus corrected should be more reliable than the original.

For the series of threshold values thus obtained (before the correction, if any is made) the standard deviation, or the probable error, or both, should be calculated. For directions see same reference to Titchener, pp. 9-11. But these calculations are very laborious. For ordinary work we therefore recommend as a measure of variation in the record the use of the mean variation (m.v.) computed as follows: Regard the difference between successive steps as equal psycho-physic steps and, with the increment which is nearest to the median as a base, multiply the number of cases which are one step from the base by 1, the number that are two steps away by 2, the number that are three steps away by 3, etc.; divide the sum of these products by the total number of cases (sets).

This record on a heterogeneous group should be regarded (1) merely as a preliminary to more accurate tests in homogeneous divisions of the group, or (2) as a rough record, serviceable when exactness is not required. It is a "quick method."

B. Final. Take in one division all observers who have approximately the same threshold, as determined by preliminary test; use one, two, three, or even four¹ increments, according to the closeness of the grouping, and proceed by the method of right and wrong cases to find the threshold for 75 per cent correct cases.

C. Individual Tests. In making individual tests the preliminary procedure here recommended for the group may be followed; but in most cases it is better to make a quick informal skirmish to find the approximate threshold and then proceed by the method of right and wrong cases, using a single increment.

V. INTERPRETATION.

A. Distribution of the Records for a Group.

1. *The Normal Curve.* The normal curve of distribution of the records for a preliminary group as determined by the preliminary method described above is shown in Fig. 1. (From data collected by George H. Mount and Franklin O. Smith.) The curve is based on the records of 781 undergraduate university students, 296 men and 485 women.

In the interpretation of this curve it should be remembered that: (1) most of the records (581) are in terms of the "average of the middle third,"² in place of the median; (2) no correction of this has been made on the ground of internal

¹ For a first homogeneous test on large numbers it is convenient to take three groups as follows: Group A, including all those whose thresholds lie above 9 vd.; Group B, including all whose threshold falls between 9 vd. and 3 vd., inclusive; and Group C, including all whose threshold lies below 3 vd. Later, for extended tests, these groups; may be subdivided.

As a rule, one hundred trials in each instrument is the very minimum for the number of trials that may be trusted. Increments for which the record shows less than 65 per cent or more than 90 per cent right cases should not be used.

² The average of the middle third was obtained by rejecting the lowest third and the highest third of the observations and counting the "average of the middle third." For 183 cases in which the median and the average of the middle third were both computed, the former gave 7.4 and the latter 7.2 as a central value. The results are therefore quite comparable.

evidence in the individual record;¹ (3) no increment of less than 1 vd., or more than 30 vd., was used; (4) the division of groups for the curve is made midway between increments; (5) the observers were without practice (the test was made in an hour) in this test, but all accustomed to psychological experiments; and (6) the observers represent a select body, as admission to the University represents a survival process.

This curve may be regarded as a norm. It may be expressed conveniently in a mathematical equation.

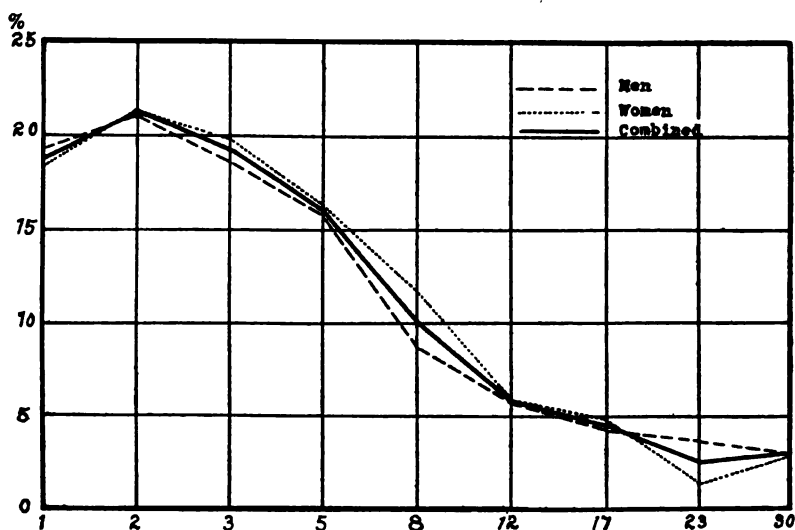


FIG. 1

3. *Comparison with Other Sensory Discrimination.* There are two characteristic features in the curve: the decided skewness, and the absence of a well defined mode. These together indicate that there is a wide range of difference in the capacity of individuals. There is no mode or unit which may be said to represent a norm. Instead of seeking an average, or norm, we must seek to establish some serviceable division into classes.

¹ Such corrections always lower the result. They may be found necessary in from 5 to 10 per cent of the cases.

In this respect this curve differs from curves representing sensory discrimination in other factors; *e.g.*, visual or tactual discrimination for linear magnitudes, form, or size; weight and pressure; and direction and intensity of sound.

4. *Extremes.* Individuals are found who can detect a small fraction of difference in pitch; these, if any in this group, are here included with 1. The marvelous keenness claimed by, and reported in the biography of, certain musicians has seldom been verified. A verified threshold of less than .3 vd. is rare. On the other hand defective tone discrimination gradually shades off into tone deafness. Such cases are included in 30 + in the above curve. Tone deafness probably prevails to the same extent as color blindness; but we have no satisfactory statistics of either.

B. Reliability.

1. *The Mean Variation.* In the preliminary test we have the m. v. as a measure of validity in the record. Fig. 2 (from Franklin O. Smith) shows the distribution of the m.v. in a group of records as figured according to the directions given above. The figures at the base denote m.v.; those in the side of the curve, per cent of error. It ranges roughly, between .5 of a step and 2.5 of a step. A m.v. of 1.0 step or less indicates an entirely safe record; a m.v. above 2.0 steps proves the record worthless; between these extremes the reliability is roughly in inverse ratio to the magnitude of the m.v. The m.v. should therefore help us in determining the necessity of verifying a record by further tests. The standard deviation and the probable error if calculated, of course, serve the same purpose in a more exact way.

2. *Internal Evidence and Notes.* In the case of a large m.v. it is imperative to examine the distribution of the observations. Two extreme types are shown together in Fig. 3 (from F. Z. Wheeler). Each is based on 34 "sets" of trials. The average of the middle third is the same in the two records, namely 5.3 vd. The median is also approximately the same, but the m.v. for A is 1.1 and for B, 3.6. Thus the record for A

is fairly reliable, but for B utterly unreliable. But the record shows that B has 8 out of a possible 34 records right on $\frac{1}{2}$ vd. If he has no capacity in pitch discrimination the probability is 1 in 1024 that he should get a record on $\frac{1}{2}$ vd. in one set, *i.e.*, the whole column right, by pure chance. And from whatever point in the set pure chance operated, the chances of getting successive judgments right decreases in geometrical ratio. Thus, if his actual threshold were really above 5 vd. the chance that he would get 5 vd. (and all above) right is

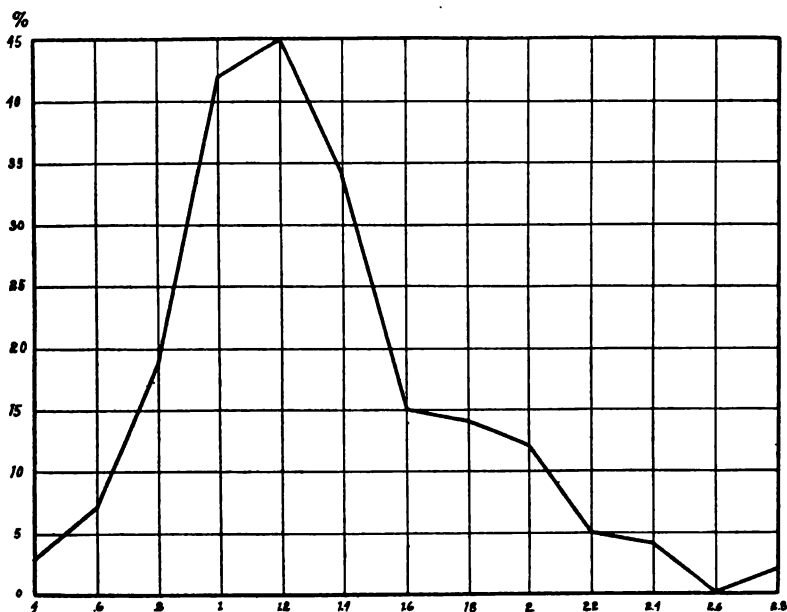


FIG. 2

1: 2; 3 vd., 1: 4; 2 vd., 1: 8; 1 vd., 1: 16; and $\frac{1}{2}$ vd., 1: 32. Eight records on $\frac{1}{2}$ vd. in 34 cases therefore almost amounts to certainty that B can hear a difference of $\frac{1}{2}$ vd. A correction must therefore be made, inserting the record $\frac{1}{2}$ vd. as one mode and the probable threshold.

But B's remarkable record may be due to one of at least two different causes. A glance at the original record shows that all the poor records were made in the first part of the test; toward the end he is almost perfect, which of course means that

in the first part of the test there was a lack of understanding of something. This adds even more assurance to our estimate of the $\frac{1}{2}$ vd. threshold. Another possible explanation of such a record, except for this last fact of progressive change, is that B had a good ear but was a "scatterbrain." It might also mean that there was some other disturbing element either subjective or objective in the test, but in no case would we have any doubt about the quality of the ear.

In accepting records we must always bear in mind the numerous sources of error—objective, in the apparatus or in the

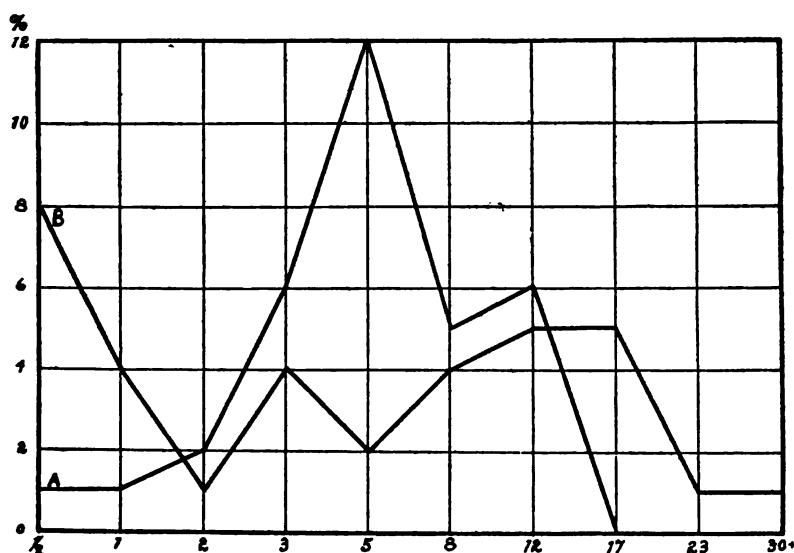


FIG. 3

manipulation by the experimenter; or subjective, due to ignorance, inattention, lack of effort, etc. The longer a careful experimenter works at it the more he learns of how conditions, apparently irrelevant, may play an important rôle. With patient practice we can learn to conduct the test on a group so that each individual record receives more than mechanical treatment. One must play over the full gamut of errors before he attributes a peculiarity in the record to the ear.

3. *"Final" Test.* In following the method proposed for the "final" test, with an individual or a homogeneous group, the reliability depends upon the number of trials, but first and last upon the favorableness and the accuracy of the control of the conditions under which the judgment is rendered.

4. *Proportionality of Increments.* It should not be assumed that the perceptibility of the difference in pitch varies directly with the difference in the pitch of the two tones. Tones that blend, such as the fundamental and the major third or fifth are readily confused. But, within the range of 30 vibrations, at this pitch, the variation is probably fairly direct.

5. *Difference vs. Direction.* The threshold for the perception of difference is of course different from the threshold for the perception of direction of the difference. While the former is the principle which operates in nearly all musical appreciation, the latter is usually taken as the measure of pitch discrimination, partly because it measures a capacity in musical expression but chiefly because it can be determined more accurately than the former.

6. *Relative Central Values.* In estimating the value of the threshold we must take into consideration (1) the form of judgment upon which it is based, (2) the step counted, (3) the kind of central value employed, and (4) the apparent plasticity of the threshold, illustrated in 7, 8, and 9, below.

The reasons for selecting the form of judgment here recommended and rejecting the threshold which depends upon subjective certainty have been stated in Sect. II A 9 above.

We have chosen to count the last judgment preceding the first mistake in a set because we have found empirically that it comes nearest to the actual threshold of just perceptible difference, when a sufficient number of sets of trials have been taken. Granting that this step is subliminal it would of course yield right judgments half of the time by pure chance, and the same is true of all steps below it, each considered by itself. On the other hand, it is well known that in this form of judgment errors will occur above the threshold for various reasons. The step we have adopted seems to strike a fair mean between

these two tendencies. Furthermore it represents a definitely definable procedure.

For central values we have at our command different kinds of averages, modes and medians. The ordinary average or arithmetical mean is out of question, since the large increments would count too heavily in proportion to the small. The average of the middle third largely obviates this difficulty and has the advantage of an average; it is therefore, quite satisfactory. The mode is not always representative, but the mode, or modes, in the distribution of a set of observations must always be taken into consideration: we count it a secondary central value, often most significant. The calculated median (not the median of plain inspection) is the most representative value that we can use because it eliminates the extremes and gives a fair distribution of the most characteristic values.

The relation of the median as obtained in the preliminary test to the record by the method of right and wrong cases, as obtained in the "final" test has not yet been worked out to a satisfactory degree of certainty. A preliminary estimate of the relationship (by F. Z. Wheeler) makes the median to be not far from equal to 75 per cent of right cases. For 225 cases the median for the one hour preliminary test is 3.3; the threshold for 75 per cent correct cases is 3.1 in an individual test immediately following. Data are being collected for a direct comparison of the two central values in the same observations.

7. *The Cognitive vs. the Physiological Threshold.* In sensory discrimination of this sort we may speak of two thresholds: the physiological, which is set by the limits of capacity in the end organ; and the cognitive, which is set by cognitive limitations. Theoretically we always aim to reach the physiological threshold, but practically we often fall short of this and find a cognitive limit; *i.e.*, a higher threshold due to lack of information, best form of attention, interest, effort, etc.; or to disturbances of some sort. Usually inspection of a record or observations made in the test enable us to tell whether or not we have reached the physiological threshold. It cannot be judged by any single rule, although a small m.v. with one

well defined mode are pretty sure indications. This distinction is of greatest importance in classification, and in the theory of training.

8. *The Illusion of Difference.* One of the most bewildering experiences of a trained observer is to find that he is subject to very strong illusions of difference. Such illusions are common, in mild form, with reference to all liminal values, but here it is very pronounced and almost unbelievable.

Closely related to this illusion is the fact that the feeling of certainty in our judgment, near the threshold, is not as reliable here as in most other sensory discrimination. An extensive record of the degree of certainty by a trained observer, on a scale of five, for a long series of judgments shows that there is no close correlation between the degree of certainty felt and the actual correctness of the judgment.

Very much depends upon the direction of expectant attention. Listening to two tones physically of the same pitch one can make either tone seem higher or lower at will. This introduces a very serious difficulty in the experiment. It is exceedingly difficult to keep the observer from directing his expectant attention in favor of one or the other. Great ingenuity and watchfulness on the part of the experimenter is necessary in order to keep the observer as neutral as possible.

9. *Plasticity of the Threshold.* Except for the very keen mind with a keen ear, the threshold is not a sharply fixed limit. In the first place there are so many obstructions to the most favored hearing that we always expect fluctuations in judgments and do not assume that we have reached the absolute physiological limit, but rather a normal approximation which may be used as a norm. For most of our purposes it is quite as serviceable. On the other hand, it is not unwarranted to suppose that the surface of frequency for a given set of records actually, though in exaggeration, represents something of the flexibility of the physiological threshold.

C. *The Tonal Register.*

1. *The Normal Curve.* Our tests have been made at one level, á, 435 vd. What knowledge have we of the relation of this level to other levels? Records have been published on individual musicians, notably Luft and Stumpf, whose discrimination is exceedingly fine, but a record of more nearly average observers is more directly applicable for us. Fig. 4

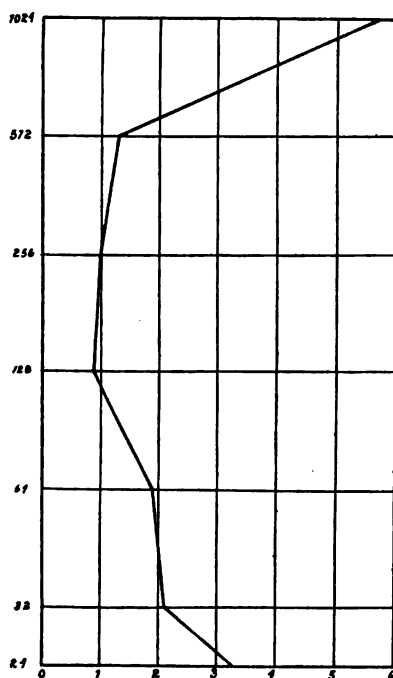


FIG. 4

(from unpublished MS by H. G. Schaefer) is a representation of the average for eleven good observers in the laboratory showing the relative sensitiveness to differences in pitch to different levels within the tonal register. From 100 vd. to about 500 vd., the least perceptible difference is almost a constant in terms of vibration frequency and a geometric ratio in terms of tone interval; but both ends of the curve

deviate from this rule.¹ In general the individual curves are of the same type as the composite, regardless of whether the threshold is high or low.

For our present purpose the curve shows (1) that measurements made within the middle of the tonal register are comparable and convertible in terms of vibration frequency (2) that a moderate increment taken above the standard is practically equal to one taken below, in terms of vibration frequency; and (3) that 435 vd. represents approximately the most sensitive level in terms of the tone interval.

2. *Gaps.* However, certain individuals have "gaps," i.e., parts of the register for which they are less sensitive than the normal curve would indicate; or, they may even be tone deaf for a few notes, although keen in the hearing of other notes. In such islands of partial tone deafness the person hears the tone just as distinctly as elsewhere and feels that he can discern pitch difference, but, upon being tested, he finds that he is more or less helpless. The writer has such a gap from 64 vd. to 45 vd., but it took a variety of crucial tests to convince him of it on account of the prevailing illusion of difference at this point. Statistics in regard to the extent and frequency of this phenomenon are being collected. Such gaps furnish most excellent opportunity for study of the traits of defective tone hearing by one who has a keen ear for other parts of the register.

D. Correlation of Pitch Discrimination with other Possible Factors in "Musical Ability."

Correlations have been worked out for musical education, environment, heredity, ability in performing, tonal fusion, appreciation, singing, rhythm, auditory imagery, auditory memory, etc., but it has not been found advisable to include results of that work in this preliminary report. Reports on this subject will be published soon by G. H. Mount and others.

¹ Luft and Meyer (or Stumpf) did not find such deviation at the ends.

E. Practice, Age and Sex

1. *Practice.* The above correlation may be summed up in the statement that pitch discrimination is an inborn capacity which ordinarily reveals itself in full force without special training just as the psychophysics capacity in auditory acuity or visual acuity reveals itself spontaneously.

The problem of training is one of great practical significance. It presents a fertile field for experiment in educational psychology. Under our direction the problem has been approached through the following, among other, kinds of practice series in which intensive and exclusive training in pitch discrimination has been given by employing the "final" test above in a prolonged series of tests (*i.e.*, the training was gained from the continuous tests): with (1) persons with good ear and (a) musical education and (b) no musical education; (2) persons with average ear and (a) musical education and (b) no musical education; and (3) persons with inferior ear and (a) musical education and (b) no musical education. These tests are made both on adults and on children, and both by individual and group training.

At this preliminary stage, the following general conclusions may be stated:

(1) When the proximate physiological threshold has been reached, practice is of no avail.

(2) So long as a cognitive threshold prevails there is prospect of improvement by practice to the extent that the cognitive is above the physiological, limit.

(3) This improvement is usually very rapid, often immediate and can usually be traced to the acquisition of knowledge, through experience or information about what pitch is, as distinguished from other attributes of tones.

(4) In the majority of cases it is possible for the ingenious experimenter to discover the proximate physiological threshold to a fair degree of certainty in a well planned half-hour individual test, or in one heterogeneous and one homogeneous group test of one hour each; and, for most of the cases in which this fails, the same tests demonstrate positively that the threshold is only cognitive.

(5) The possibility of reaching the physiological limit in a single test depends to but very slight extent upon whether or not the person tested has had musical education; it is mainly a matter of expert skill and ingenuity on the part of the experimenter.

(6) Ordinarily musical education is not effective as a means of improving pitch discrimination. (From work of H. S. Buffum, F. Z. Wheeler and George H. Mount).

As an illustration of the degree of finality which may be obtained in a preliminary test we may turn to Figure 5 (from H. S. Buffum). Dr. Buffum experimented on twenty-five eighth grade pupils in a grammar school room. He first made a fifteen minute individual test of each pupil and classified them on this basis into three groups with modes at 3, 8, and 17 vd. respectively. The object was twofold: (1) to determine the effect of practice and (2) to determine the success of the preliminary examination. For this purpose he gave them twenty forty-minute periods of training.

The training consisted in taking records by the method of right and wrong cases. The figures at the bottom of the curve denote days, those on the side per cent of right cases; the fourth curve is a combination of the three. The results show (1) that for no group is there any evidence of improvement with this practice, and (2) that all except two children remained throughout the whole practice series within the group to which they had been assigned. Of these two, one who had been assigned to group III was immediately found to belong in group I as there had been a failure to understand the preliminary test; and the other, although retained in group II, proved really to be near the dividing line and could have been classified in group III. Evidently the physiological threshold had been reached in twenty-four of the twenty-five cases in the preliminary test.

The capacity for appreciation of music is partially inborn and partially the result of training. Thus, in judging the quality of an instrument or a voice, the expert hears and observes differences and peculiarities that entirely escape the untrained ear; and all differences in so-called quality or tim-

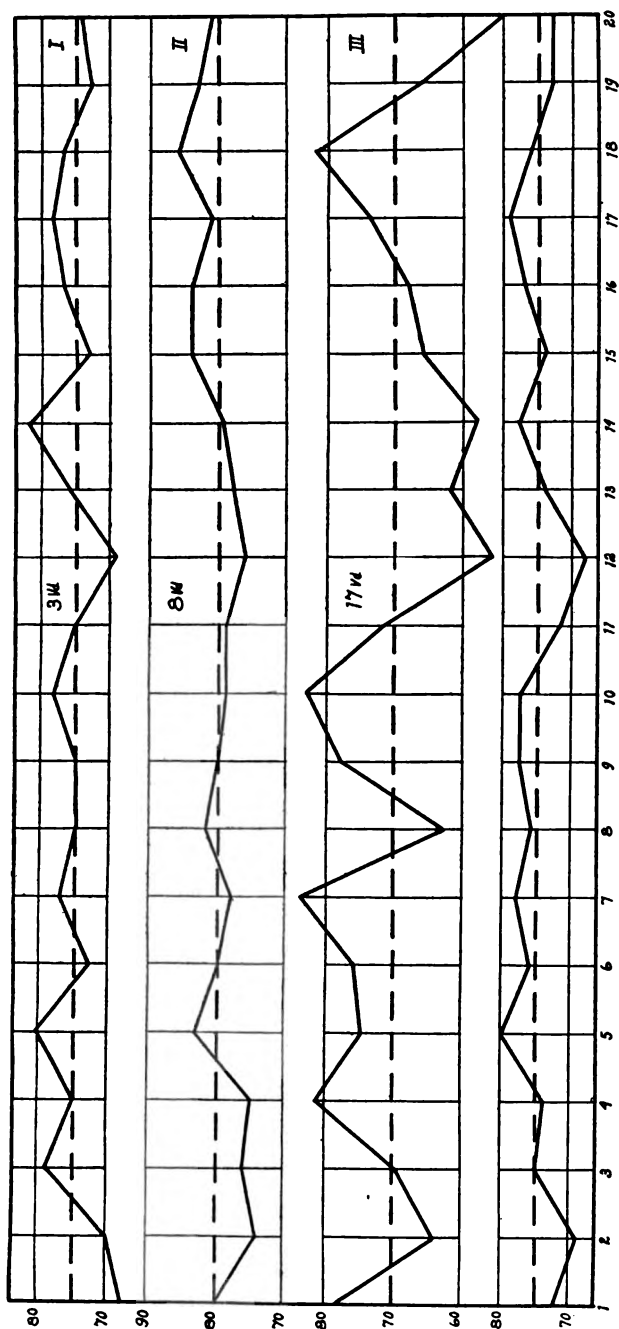


FIG. 5

bre of tones are reducible to pitch. But such hearing represents a complex process of interpretation, which can be mastered only after extensive training. The mere detection of pitch difference is, on the other hand, a simple process requiring only the slightest amount of training.

Now, to the extent that we control and simplify conditions so as to make our test a simple, sensory, direct comparison in regard to which there can be no misunderstanding, we have a valid test of pitch discrimination which approaches the physiological limit.

2. *Age.* A preliminary estimate of data on the variation of pitch discrimination with age may be summed up in the following general conclusions:

(1) The physiological limit is probably lowest in early childhood but remains fairly constant up to maturity.

(2) In a bright child with a good ear the physiological limit can be established for all practical purposes as early as the age of five.

(3) The slight inferiority of record which we find in a group of young children over the record of a group of adults is due to cognitive difficulties, which are not in the nature of a lack of skill in the sense of a slowly acquired ability but rather due to lack of knowledge. The university students have every advantage—musical education, maturity for reliability in observation, power of application, familiarity with the experimental conditions, etc., which is quite enough to account for the superiority of their group record over the record of a group of children. There is no such large difference of advantage between high-school pupils and grammar-school pupils, and their records practically agree.

3. *Sex.* Pitch discrimination does not vary in any constant manner with sex. The small oscillating differences shown in Fig. 1 are characteristic.

F. Practical Use.

1. *In the Psychological Laboratory.* The main use of this measurement is of course in the psychological laboratory—

in teaching, and in research. But the discussion of that feature scarcely comes within the scope of this report.

2. *In the School Room.* While this is a "mental test," our report demonstrates that we are not justified in using it as a general mental test, or test of intelligence, as has often been done. It gives us a measure of a specific and peculiar capacity—probably dependent upon the structure of the sense organ; and that is a reason for employing it in the school room in this age of interest in individual capacity and adaptation.

Suppose that we find four children of equal age, advancement and general ability sitting together and one has a threshold, for pitch discrimination, of $\frac{1}{2}$ vd., another 3 vd., another 12 vd., and another 25 vd. They are to have singing lessons. How can we group them properly for this period? Nine years ago the writer proposed the following arbitrary classification as a tentative measure (*Educational Review*, June 1901):

- Below 3 vd.: May become a musician;
- 3-8 vd. : Should have a plain musical education (singing in school may be obligatory);
- 9-17 vd. : Should have a plain musical education only if special inclination for some kind of music is shown (singing in school should be optional);
- 18 and above: Should have nothing to do with music.

This might serve as a basis for the adaptation and assignment of singing exercises, or substitutes for singing.

Exercises of this sort should be a part of the music course in school. They furnish the best kind of mental discipline; they direct attention to one feature in music in a concrete way; they stimulate interest and effort in observation and right execution of pitch; and, incidentally, they furnish instructive records which may be collected as a basis of classification. Children are entitled to know the facts thus learned. One period set aside for the interpretation of the records may be of untold value especially (1) to those children who are in danger of having a musical education forced upon them, although they are physically incapable of hearing music in

the true sense; and (2) to those children who first discover in this test their natural ability in this respect and are stimulated to take an interest in music.¹

3. *In the Studio.* The first music lesson should be a pitch test. There are scores of other factors which go to make up musical ability, but unless a person has a good ear, he will fail to appreciate and will be unable to produce music which depends upon the ear. We often find music students who lack a general interest in music because they have no "ear."² A person may have a good ear and yet be lacking in other musical capacities, for example, rhythm, hearing, consonance and dissonance, auditory imagery, affective tendencies, etc. Therefore, this pitch test should be followed up with other tests on musical capacity, but this is the first and fundamental.³

4. *General Cautions.* As this measurement constitutes a quantitative rating of a capacity of an individual in such a way that it may act as a powerful stimulus or an effective deterrent, we cannot be too emphatic in demanding that information about rating and advice based upon it should be given not only conservatively but with prudence and reserve. In one case the rating may blast a life ambition, in another, it may be the means of discovering the individual.⁴

¹ The place for the test is in the first six grades. It can readily be adapted to the ability of the children of the different grades. Thus the test should be short and answer may be made by simple means, such as showing of hands and counting the number of mistakes; but for the purpose of classification greater care must be taken. As a rule, the younger the child the more necessity there is for making the test individual.

² What a blessing to a girl of the age of eight, if the music teacher would examine her, and, if necessary say, "much as I regret it, I must say that you would find music dull and difficult, and I would advise you to take up some other art." What a blessing if that child could be started right; but current theory and practice is against her. There is too much faith in what music lessons can do for a person without native capacity. If we are to have musical ears, we must be born with them. That is the probable finding of current research.

³ Among such tests in progress at the present time are the following: the sense of rhythm, and rhythmic action, tonal fusion (consonance and dissonance), auditory imagery, auditory memory, discrimination for intensity of sound, and vocal reproduction of a tone.

⁴ The writer recalls the case of a university junior who had taken music lessons for twelve years, but took no real pleasure in it and made but little headway. When she found that her rating was 12 vd. she exclaimed, "That is it. That's what I have thought," and quit music.

And in such weighing of evidence we must ever bear in mind that there is a compensation for this lack of ability in hearing fine pitch differences. An inferior ear hears tones as tones, but fails to hear small differences in pitch and may fail to hear the true pitch of a clang; but this may be an advantage as, *e.g.*, in listening to instruments out of tune or inferior rendition. He may enjoy his own singing even though he cannot carry a tune. While such partial tone deafness prevents the critical and most varied enjoyment of music, it permits and even encourages idealizing and undisturbed, crude enjoyment of tone productions provided the individual has some "music in his soul."

We must also distinguish between advice to possible musicians and to others who may desire to have some music for "home consumption." There is an element of music in the bones, the drum, and the tambourine. The associations with music are often rich and valued in themselves. Even a bare intellectual familiarity with matters musical has its charm. The words of songs get a setting from music even in the unmusical ear. We must, therefore, bear in mind that pitch is not the only avenue to musical appreciation.

In conclusion, then, let us post the following warnings:

- (1) A record should not be the basis for rating and advice until it represents the proximate physiological threshold.¹
- (2) No classifying tests should be employed with groups except by persons who are capable of taking proper experimental precautions and making a safe interpretation.
- (3) Whenever possible, rating should be considered personal, confidential information.
- (4) Persons with poor rating should not be discouraged in their enjoyment of music; there is much in music besides

On the other hand, it should not be supposed that the parties concerned always take advice. The writer was called in to tell why the oldest of three children in a musical family was not making satisfactory progress with her music; the younger children clearly outclassed her. Pitch test showed: boy, age 11, 1 vd.; girl, age 15, 1.2 vd.; girl, age 19, 13 vd. Explanation and advice was given on the basis of this rating, but the parents sent the oldest girl to a better conservatory.

¹ The cases left in suspense are no worse off than they have been heretofore. With further patience they may be reduced.

pitch of tone, and much enjoyment of music does not depend upon the hearing of fine distinctions of pitch.

(5) Although perhaps the most fundamental, pitch discrimination is only one of a score or more of technical tests which, taken together, constitute a measure of "musical ability." Conclusions must be limited to the element tested.

METHODS FOR THE DETERMINATION OF MENTAL IMAGERY.

BY JAMES R. ANGELL.

PART I—EXAMINATION OF METHODS IN USE.

In few fields of psychological investigation has the situation changed so completely in the last few years as in that covered by the title of this report. When the writer in 1906 undertook to make a comparison of the tests then in use, the literature relating to such work was relatively circumscribed and the devices then in vogue relatively simple and supposedly fairly accurate.

The author imagined, as did others, that his task would consist in making a careful comparison of the recognized tests, eliminating untrustworthy features, possibly adding a few new ones, but at least standardizing the tests in terms of one another. Although this seemed a fairly large contract, it appeared to be quite definite and was entered upon with an optimistic spirit.

Since that date a large amount of excellent work bearing upon the subject has been produced and complexities have been revealed previously little suspected. This development was foreshadowed by the first investigations made by the author in 1907 and indicated in the preliminary report made to the association at the annual meeting that year. In attempting to compare the relative merits of several familiar forms of procedure, he became convinced that the problem was much more baffling than had ordinarily been recognized and much broader in its outlook than had been admitted. The present situation is far from satisfactory, as will be made clear below, but the author herewith offers certain recommendations for tests in accordance with the promise made at the meeting of the association in 1909.

It is not without interest to remark in passing that the period during which we have heard most about imageless thought and about the fallacious importance attributed to imagery in general, should coincide with the period in which many of the best and most exhaustive studies of imagery have been made.

Before proceeding to present the recommendations, it will be desirable to pass in critical review the several forms of procedure at present in use in connection with the study of imagery. This survey is not offered as exhaustive of all variations in detail. But it includes the main forms of which the author knows, so far as he has found them productive.

The methods employed to diagnose imagery fall into two main divisions (as regards their purpose, if not as regards their results¹); (1) objective, in which the subject is set to perform a certain task so devised that the results he achieves will disclose forthwith the kind of imagery he uses. (2) Subjective, in which by various devices the subject is given tasks to perform which will presumably facilitate his ability to discover and report the imagery he uses. Evidently the first form of test is the ideal thing, for, if successful, it avoids the unreliability of merely introspective analysis and description. Objective methods may be, and ordinarily are, supplemented with introspective analyses, so that combination subjective-objective methods, rather than those of purely objective character are in general to be distinguished from the purely subjective modes of procedure.

DIVISION I. OBJECTIVE METHODS.

A. One of the earliest and still most frequently used of objective tests is that in which material is given to be memor-

¹ Several other classifications have been proposed and a different meaning is attached to the classification here used by Lay, in his monograph on Imagery (*Psychological Review Monographs*, vol. ii, no. 3), who, as I understand him, makes the term subjective apply to methods in which the subject exercises his own introspective analyses. The term objective he applies to all methods in which there is any effort at experimental control. Dr. M. R. Fernald in a forthcoming monograph recognizes three groups, (1) subjective, (2) objective and (3) a combination subjective-objective class.

ized visually, or aurally, or with the assistance of motor activities as in spelling and writing. The form in which the memorizing is most effective is assumed to indicate the preferred and dominant type of imagery.

This procedure has been modified in various ways, *e.g.*, by offering to memory objects, sounds, and words, the latter designating sense experiences of the eye, the ear, the muscles, etc. The terms best remembered are again assumed to indicate the ideational type, visual characters being best remembered by a visualizer, auditory characters by those who chiefly employ auditory images and so on.

Tests of this type may have an undoubted practical significance in connection with studies in economy of learning and permanency of retention. They may certainly be so administered as to show over what sensory arcs the best results may be achieved in assimilating information of various kinds. They may convey information as to dominant interests. They also possess an unquestioned value as a stimulus to introspection. But as objective tests of imagery, apart from introspection, they have few virtues and no reliability. For instance, a subject may learn a series of objects visually as rapidly as he learns them from hearing their names pronounced, and yet his recall of the object may be in terms of auditory-motor words. He translates the visual objects into words and at times recalls better such a series than one learned originally from words themselves. The writer is himself an illustration of this type. Were the test employed to give objective information in such a case, it would clearly result in a false conclusion. Numerous other difficulties of this same variety are met with in the effort to use the results objectively.

B. Methods of (1) Distraction or disturbance, and (2) of 'helps.' (1) The theory on which these tests proceed is old in experimental psychology, but relatively new in its application to the study of imagery. The distraction test as used, for example, by Cohn,¹ has many variant forms and

¹ *Zeitschrift f. Psychol. u. Physiol.*, 1898, vol. iv, p. 161. Cf. also Meumann, *Vorlesungen z. Einführung in d. Exper. Pädagogik*, Leipzig, 1907.

has been employed in combination with a number of other methods here mentioned. Its application may be illustrated in the realm of sound.

If a person who relies mainly upon auditory imagery is memorizing a series of words presented visually, it is assumed that a stimulation of the ear will increase the difficulty of his learning much more than it does for a learner of visual type. Similarly, distracting visual stimuli will react most disadvantageously upon the memorizing of one of visual type, and motor activities of the vocal mechanism or the hand, will most hamper one of motor type.

The assumption is broadly true on the positive side; that is to say, users of the corresponding imagery are often seriously embarrassed by such distractions, but in practice it is found quite impossible to infer, *e.g.*, that one who is disturbed by an intruding noise is therefore a person who uses auditory imagery predominantly, and the amount of the disturbance caused is in no wise regularly related to the amount of auditory imagery employed. The general distraction of attention produces anomalies in the results which render the method highly ambiguous except when conjoined with adequate introspection. Moreover, it is found quite impossible in the case of some individuals to devise any distraction which will measurably decrease the use of their preferred forms of images, short of such a distraction as will preclude any learning at all. Furthermore, cases have been found by Dr. M. R. Fernald in the Chicago Laboratory where a vocal distraction has emphasized rather than impeded the auditory-vocal imagery used in learning and recall. The vocal distraction has on the other hand proved very disturbing to the visual processes with certain subjects. In my own case such a distraction appears to be effective not so much by its stoppage of the articulatory accompaniment of the learning (it stopped this very imperfectly), but because it breaks up the associative processes of perception, so that I do not apprehend the meaning of the word easily or correctly.

It may be added in passing that although relatively sat-

isfactory auditory and motor distractions have been determined, equally satisfactory visual distractions have not been discovered.

(2) The method of helps is even more ambiguous in practice, although equally simple and obvious in theory. The commonest form of the test is in connection with such tests as were mentioned under I A. Suppose several series of digits are to be learned. A person of motor type would presumably be most helped by being allowed to pronounce the digits, aloud or under the breath, or possibly by being allowed to make the movements of writing them. The auditory imagery as judged by many memory tests would presumably be helped by a rhythmic arrangement of material. Visualizers might be helped by arranging material in certain groups or patterns.

As in the method of disturbance or distraction, the general assumption underlying this program is probably correct, *i.e.*, that there are variations in procedure which will assist particular individuals because of their peculiar imagery. But the assumption as applied is much too simple and schematic to meet the complications of actual mental life. For example, our experiences in the Chicago Laboratory seem to show that persons who use a good deal of visual imagery may be quite as much helped by rhythmic arrangement of material as audiles, and an occasional audile is disturbed rather than helped by rhythm, at least by those imposed by the experimenter.¹

Speaking well within the mark, then, one may say that the methods of helps and distractions when taken apart from introspection, are quite untrustworthy as thus far developed. Both are, however, extremely useful in stimulating introspection by throwing into strong perspective the processes actually employed in learning a given material.² It must

¹ The terms, visualizer, audile, etc., are simply used to indicate persons who in the test under discussion belong to these groupings. The ambiguity of such terms for general classification is dealt with at a later point.

² Both may be used either with children or with adults in a way to throw valuable light on the conditions practically desirable for learning, but these merits must not be confused with the objective adequacy of the method for revealing imagery.

also be clearly and emphatically stated that the objective results of these tests often afford most striking confirmation of the introspections.

C. *Kraepelin's test.* A test of Kraepelin's to which certain writers have assigned objective value consists in asking subjects to write for five minutes lists of words designating objects primarily characterized by color, in another five minutes objects primarily referring to sound, etc. The lists when compared are supposed to indicate the scale of distribution for different kinds of imagery, the dominant type being represented by the most extended list. I do not find that Kraepelin himself ever laid claim on behalf of the test to objective validity of imagery analysis. He speaks of it as giving interesting and suggestive information about mental organization.¹

It undoubtedly has some value as indicative of verbal imagination, for the test can be, and often is, carried out by subjects purely on the basis of verbal imagery. In my own experiments it has appeared to possess, as nearly as possible, no value whatever for the objective determination of type, or for the objective ascertainment of the presence or absence of any special form of imagery.

To mention only one of several ambiguous cases illustrative of the difficulties the method presents, we may notice that such a word as 'bird' is suggestive of visual and of auditory qualities in essentially like degree, and the presence of such a word in a list of reactions would leave one quite unable to decide whether it indicated auditory or visual imagery in the reagent. It is surprising to find how many terms suffer from similar kinds of ambiguity.

D. *Method of 'Style.'* The same approbation and the same criticism may be accorded the so-called method of 'style,' which is supposedly available for the determination of types through an examination of the vocabulary employed in a writer's works, especially when these are of a descriptive character. The method *may* often give correct results within

¹ *Psychol. Arbeiten*, 1895, vol. i, p. 73.

the narrow field to which it applies, but it is never possible to be sure of these and the outcome is certainly misleading in its indications in many instances.¹

E. Tests on Imagination Rather than Imagery. Paradoxical as it may sound, there are in use several forms of test sometimes used in a purely objective way, which are known as tests of imagination, although little or no imagery need be actually involved in performing them.

The 'ink-blot' test is a case in point. Ink-blot is shown to the subject and he is asked whether the blot suggests any object to him. If he replies promptly and perhaps sees several things in the blot, he is credited with an active visual imagination. If he fails to do so, he is alleged to be deficient in visual imagery. The data are too few to say in what degree the actual possession of vivid and profuse visual imagery is correlated with expertness in this class of test. But one thing is entirely certain, that persons may make a very respectable showing in the test who employ a relatively slight amount of visual imagery.

These tests and others like them are really tests of perceptual fertility rather than tests of imagery in any proper sense.

Summary.

The preceding paragraphs are offered as showing conclusively that no purely objective methods of imagery analysis are at present reliable. There may be other tests of which the writer has no knowledge that satisfactorily fulfill the necessary conditions. In any event full recognition must be accorded the valuable confirmation of introspection frequently afforded by tests of this type.

DIVISION II. COMPOUND SUBJECTIVE-OBJECTIVE METHODS.

Many of the following methods are at times used purely subjectively, though they all contain elements capable of use for objective interpretation. But, as in the preceding group,

¹ Cf. A. Fraser, *Amer. Jour. Psychol.*, 1891, vol. iv, p. 230.

none of them possesses any serious significance apart from the introspective evidence gained by it.

A. The Questionary. The method of Galton, modified and elaborated by many subsequent psychologists, is too familiar to require extended description. Specific questions are set which invite the production of images from each sense department, and the subject's introspections are recorded. Quality, number, vividness, objectivity, persistency, controllability, etc., are reported upon.

Dream imagery may be treated in the same way and many interesting experiments have been made on the control of dreams, the relation of dreaming to depth of sleep and so on; but the subject seems to be a trifle aside from the main purpose of this report and will not be further considered at present.

In the hands of competent introspectionists a good questionnaire gives quickly a fair view of the capacities of the individual for the voluntary arousal of imagery. The method is too difficult for occasional persons untrained in introspection. It is probably rarely successful in informing us with accuracy or certainty of the forms of imagery actually most employed and their manner of employment in common mental tasks. Moreover, we cannot judge that specific images are entirely lacking because they are not reported in replies to a questionnaire. Other forms of test frequently bring out images which escape notice in the questionnaire method.

B. Association from a Stimulus Word. A series of words is shown or read one at a time and the subject at once reports the ideas called up and the imagery involved. If the stimulus words are skillfully collected, the results are often very satisfactory in the calling out of various forms of imagery and in giving indications of the type prevailing for this kind of task. The reactions themselves, as given in words, are often suggestive, though by no means adequate for an objective test. The test is less significant for persons who use chiefly word imagery, and it may lose value still further in the case of the 'automatic' reagents for whom a reaction word comes solely and immediately as a suppressed enunciation.

This is the main limitation of the method, that it tends to magnify word imagery. Of course it gives no adequate indication of the distribution of imagery in ordinary activities.¹

C. Method of Letter Squares. This method has been employed with sundry modifications by several psychologists and in its most frequent form is known by Binet's name. Letters are presented in a frame-work of squares so arranged as to bring the squares into vertical and horizontal lines like a chess-board. In the simplest form of the test the subject reads the lists a certain number of times visually and then attempts to repeat them. Full introspections are invited.

It is commonly assumed as a matter of objective import that a visual-minded person can reproduce the series in orders other than that in which the list was presented, more easily than one using no visual imagery. It is further assumed that the nature of the errors made will reveal the character of the imagery. If confusions are made where the appearance of the stimulus letters is similar, as E and F, while the sound is different, visual imagery is assumed to have been used. If the confusions are of letters of similar sound, such as Q and U, it is presumed that auditory imagery was involved.

This method, apart from the many complications which have been introduced into it—in my own work, for instance, where it has been combined with features such as distractions and helps taken from other tests—shares with the questionnaire and the word methods the advantages of stimulating introspection in a very useful manner. Like the word method it is somewhat superior to the questionnaire in simulating more nearly a normal mental task and its results are accordingly more likely to be indicative of ordinary imagery processes.

The objective features of the test are quite untrustworthy. For instance, persons who use visual imagery, but use it poorly, may be able to visualize the separate letters in memory, but be quite as embarrassed to reproduce them in an order other than that in which they were learned, as persons

¹ The test in this form is known by the name of its inventor, Secor. *Amer. Jour. of Psych.*, 1899, vol. xi, p. 225. The literature dealing with the general problems of association reactions contains much available material.

using auditory imagery. Moreover, persons using auditory-motor imagery are by no means unable to vary the order in reproduction. The method as generally used affords no adequate means of distinguishing auditory from kinaesthetic ideational material, leave alone the distinction of sensational from ideational kinaesthetic elements. Again, the confusions made are frequently ambiguous, so far as concerns indicating the imagery employed. When the letter B is confused with P, and M with N, it is evident that either auditory or visual similarity would afford an adequate clue. On the other hand the confusions are often extremely suggestive.¹ Owing to the remarkable facility with which some individuals 'shunt' from one form of imagery to another, or use two or more forms conjointly in a given test, as will presently be further explained, and especially in view of the ease with which they may use words as substitutes for objective forms of imagery, it is impossible to infer with confidence the domination of a given kind of imagery merely from the objective results attained.

A case peculiar to my own learning processes in this test may illustrate another possible ambiguity. If I am obliged to repeat the words before they are completely learned, my recall is likely to be dominated by visual processes. On the other hand if I am allowed to proceed until the learning is quite perfect, the recall is likely to be mainly in auditory-motor terms, and the more perfectly automatized the act becomes, the more I lose the visual element. Judged at one stage of the process, I should then be set down as a visualizer; judged at another stage, I should be auditory-vocal-motor.

D. General Tests on Memory and Sense-Discrimination. As these tests just mentioned concern primarily visual and auditory imagery with a subordinate recognition of motor images, it may be remarked in passing that all tests on memory can be employed to throw light on imagery analysis,

¹ On the general subject of linguistic lapses as it bears on this subject, see, Bawden *Psy. Rev. Monog.*, 1900, vol. iii, No. 14.

and most of the recent memory investigations have been characterized by detailed description of the imagery discovered. Nonsense syllables, numbers, words, letters, colors, tones, noises, every kind of material has been studied in this way.¹ The differences between immediate and mediate recall, between mere recognition and independent recall, between memory and imagination, have been pointed out for many cases. Tests on sensory discrimination have been similarly employed. Wolfe's tests on tonal memory, and Lehmann's tests on the discrimination of grays, may serve to illustrate the point.²

The comments made on the merits and defects of the immediately preceding test apply here too, and need not be repeated.

E. Tests Based on Description. Tests of this character have been used for various purposes and are clearly applicable to the investigation of certain phases of imagery. The tests can be complicated indefinitely especially by combination with distraction and help devices. The observer is shown an object or a picture, preferably one in colors, for a definite period, *e.g.*, ten or fifteen seconds, and then at once, or at a later time, is asked to describe what was seen. Introspections are then asked concerning the process during the original stimulation and that involved in recall. The test proves in practice to elicit a good deal of non-verbal imagery not easily stimulated in other tests.

To illustrate: a series of words memorized by vision, may be recalled almost wholly through vocal-kinaesthetic imagery, thus leading to the inference that a subject uses no visual memory material. The same person if given a picture to observe and subsequently describe, may find abundant vis-

¹ Several experimenters have commented upon their observations that visualizers tend to learn more slowly than audiles. The experiments carried on in the Chicago Laboratory have not confirmed this generalization. Several of the quickest learners have been visual, and no general results have been reached which substantiate this view.

² Wolfe, *Phil. Studien*, 1886, vol. iii, p. 534. Lehmann, *Phil. Studien*, 1889, vol v, p. 96.

ual imagery present. It will often be found extremely illuminating to allow the subject to see the object again after he has given his description.¹ The discrepancies between the stimulus and the memory imagery are often most suggestive as to the influences under which imagery is built up and the second inspection is remarkably helpful in inciting the subject to greater accuracy.

In this connection an interesting confirmation of the extremely subtle shifts in imagery resulting from slightly altered conditions has been observed by Dr. M. R. Fernald. Observers have been found whose imagery alters to a considerable extent when they know before the experiment that a description of the picture or object is to be asked of them, as compared with their procedure when they are ignorant that such a request is to be made. In the former case words are more likely to be used during the inspection of the picture or object and the recall may be largely in word form, whereas otherwise the whole process—inspection and recall alike—may be dominantly visual with little or no evidence of verbal material.

Skillfully conducted, the test is an excellent one which can be made to simulate with practical fidelity the conditions of common mental processes. Its limitations are simply those incident to all subjective tests, *i.e.*, difficulties in introspection and lack of generality in the results. The imagery employed in describing one set of visual experiences, for instance, may differ in many particulars from that used in recalling another set.

F. Tests on Reading. We may mention next tests involving reading, (a) aloud by the experimenter, the subject listening, (b) by the subject either silently or aloud with reference either to (1) immediate introspection on the process of apprehension, or (2) also and primarily with regard to the mode of recall and its accuracy, whether tested at once or after an interval, whether for verbatim transcript, or for the giving of the general meaning. It is ordinarily advised that

¹ Except in vision the test is not significantly different from memory tests elsewhere discussed and we therefore mention only the visual case at this point.

the materials for the purpose should cover a considerable range of intellectual interest and should include both prose and verse.

An interesting variant on the test which has often been used as a test on verbal imagination, and as a test on general intelligence, consists in the submitting to the subject prepared sentences in which occasional words or parts of words, or even whole phrases have been left out, and the subject is called upon to supply relevant material for the gaps. Sometimes nouns and verbs are given and the subject must make a sentence. This is similar to the old word-building game where the prize goes to the competitor who can make the most words out of combinations of the letters in the stimulus word.

Obviously the same limitation belongs to the tests of the immediately preceding paragraph as that which we have already pointed out in connection with the test I E, namely, that certain subjects can perform them using little or no discernible imagery and that with many persons introspection is extremely difficult. On the other hand for persons of strong auditory-motor verbal tendency the tests may prove very useful.

This general group of tests in any of its forms is essentially subjective, so far as concerns its contribution to the analysis of imagery; but many secondary objective indications are found in the actual procedure. It is a distinctly useful group for practiced introspectionists, though in general unduly difficult for untrained persons. Its greatest merit is that it employs common and fundamental forms of mental activity for investigation. When successfully controlled, it gives information bearing directly on actual norms of mental conduct. It is easily complicated by distraction and help methods, often with excellent results in the facilitation of introspection.

G. Tests on Sensory Similarity in Recall. This test has been devised by Dr. M. R. Fernald and, so far as I remember, has not been previously used for this purpose. The test has various forms. In one form the subject is given a word,

say 'tone,' and asked to write all the words he can which rhyme with it. His introspections are asked and especially as concerns any words which suggested themselves and were rejected. Again, a subject is asked to write all the words he can recall which end with certain letters, say 'ine' regardless of the pronunciation of the word. In other forms of the test lists are prepared either with or without a letter-square frame, in which words pronounced alike, but spelled differently are to be memorized. In other lists words spelled alike, but pronounced differently are dictated. Here, as in the distraction tests, one might assume that persons using visual imagery would fare best in writing lists of words ending in the same letters, and those with auditory imagery would do best in writing lists which rhyme. Again one might anticipate that audiles would be more confused in memorizing the lists containing words pronounced alike and spelled differently, whereas the visualizer would fare worse on words spelled alike and pronounced differently.¹ But like all attempts at objectively valid tests, the results here are untrustworthy. Nevertheless, the 'rhyming' brought out auditory or vocal-motor verbal imagery in practically every case examined, and persons who use such materials normally found it present here in great distinctness and profusion.

If persons were rigidly confined to the use of a single kind of imagery, objective tests of this variety might be practicable. In point of fact, however, as we have repeatedly noted, individuals are rarely or never so circumscribed as regards their available imagery that they cannot at need fall back upon some variety not regularly used, or by calling in supplementary aids manage to master the situation. For example, an individual who commonly uses visual imagery for purposes of recall of objects may, in the recall of words, spelled alike but pronounced differently, resort either to the

¹ Such words as the following have been used: cite, site, dew, due, scent, cent, sent; lead, as a verb and as a noun, dove as a verb and as a noun; similarly the words wind, row, tear, wound, and the following words, which also have two pronunciations, close, bass, live, vase, gill. It should be said that Dr. Fernald undertook no objective application of the test.

use of auditory and kinaesthetic imagery, or to the making of supplementary associations of a helpful kind based on meaning. The objective result can consequently never be used apart from introspective confirmation.

As a stimulus and aid to introspection the tests are extremely useful; especially as regards the exhibition of shunt systems in imagery, by means of which commonly unused forms or devices may be successfully enlisted when occasion requires. The work is sufficiently akin to normal mental tasks to give the introspections genuine significance. Like all the tests thus far noticed, with the possible exception of the questionnaire, disproportionate emphasis is probably given to visual and auditory materials at the expense of motor imagery other than the linguistic type, whereas the remaining forms of imagery are practically neglected entirely.

Dr. Fernald has used the test in connection with letter-squares employing in pairs words of the kind described and adding distractions, but no further comment need be made on the case.

H. Tests Involving Spelling. These can be used in a number of ways, the words being spelled orally, or in writing, with eyes shut or open, spelling forward or backward. Tests on backward spelling seem to have been used less than one might anticipate. Like the previous test, this type may be complicated with distraction and help devices.

It is an admirable form of test to use in conjunction with tests on description, in as much as it brings into the foreground peculiarities of verbal imagery as compared with the imagery of objects. This virtue constitutes also its main limitation. Other forms of imagery are notably infrequent with most subjects.

I. Tests Involving Simple Problems. Simple arithmetical problems may illustrate this method. The addition of numbers seen or dictated, the multiplication of others, serve to give material for introspection. The problems can of course be complicated indefinitely and can be connected with practical situations after the manner of the text-books on arithmetic. Geometry and Algebra can be similarly drawn upon.

Tests on geometrical forms are peculiarly successful in throwing visual imagery into the foreground and algebraic problems often emphasize motor elements. Mental manipulations of the cube have been frequently employed: e.g., think of a cube; cut it through from top to bottom on a line joining two diagonally opposite corners; what shapes have the resulting pieces? Problems in ethics, in physics, or in deportment, anything one chooses may be employed, and when the conclusion has been reached, the subject is asked to describe as fully as possible the mental material used in reaching a solution. Or he may be unexpectedly interrupted during the process and asked to report his introspection.

For persons unpracticed in introspection the problems must be very simple. (The 'imageless thought' advocates must be reckoned with in interpreting the results of any such tests). There is always danger that the imagery recorded may not have been essential to the solution, but rather a fringe of more or less irrelevant suggestion. If this danger can be safeguarded, and only by accurate introspections can it be, the test can be made very useful, and it has as not the least of its merits, the normality of the tasks set.

J. Tests with Writing. A great deal of experimentation on the processes of writing is more or less directly available for purposes of analyzing imagery. When so applied, the aim is of course to devise situations in which the imagery employed may be thrown into the foreground of consciousness so as to be introspectively available. Writing from dictation, from printed or written copy, from memory; writing prose or poetry, writing not only forward but backward, and also upside down, left-handed, and in looking-glass script, writing amid various distractions, all have been tried and with most interesting results. These tests lead naturally to the mention of a large group of tests devised primarily, as were the foregoing, for the study of voluntary control.

K. General Tests on Voluntary Control. Here may be mentioned in addition to the tests on reading and writing, tests on the use of habitual coördinations such as are concerned in speech, in walking, in grasping objects, in muscular reaction

to various kinds of stimuli, and especially, tests on the securing of control over new coördinations. The latter may be illustrated by the experiments on learning to move the ear, learning to move the final joint of the fingers, independently of the lower joints. The learning of acts of skill such as type-writing, skating, bicycle riding and the like, represent another important group.

All of these tests, except those on learning to control new muscles, enjoy one merit in common, *i.e.*, that they represent in the most concrete and tangible form ordinary kinds of occupations which are largely free of the merely laboratory flavor. They all suffer too, under a common disability, *i.e.*, that for unpracticed observers, the introspection is often extremely difficult, and sometimes essentially impossible. Many sensory elements are inevitably interwoven with the ideational ones, and this adds a very distracting complication which destroys confident introspection on the imagery for not a few observers, even of the practiced sort.

L. Tests for Kinaesthetic Imagery. The experiments just referred to on voluntary control frequently bring into marked prominence the presence of motor imagery so-called, and most of the tests previously mentioned serve to disclose the part played in mental life by the articulatory imagery. It might seem that any further mention of motor imagery would be superfluous. (We waive here, as previously, the question whether such alleged imagery is or is not invariably connected with muscular excitation, with the sensory results of which it is merged.) But it is worthy of mention that in tests specially devoted to the analysis of motor memory we sometimes secure clearer evidence of this type of imagery than in such tests as we have heretofore described, in which attention is in the nature of the case less immediately directed at the motor act, and more immediately directed at the objective results of the act. Tests on the estimation of the extent of successive arm-movements may illustrate the point. Many subjects to be sure, make judgments of this kind by means of secondary criteria, such as the time and rate of movement, or by immediate discriminative reactions, in

which no kinaesthetic imagery appears. But others resort very definitely to the kinaesthetic image, or to something which they regard as this image, and compare with it the second reaction.

Obviously any muscle group may be submitted to tests of this character, and any exploration of motor imagery which is to be complete must make some such general survey before it can be accepted.

A further form of motor test which has been studied in the Chicago Laboratory on several occasions consists in allowing a blind-folded subject to trace with a short pencil patterns of various designs cut out of wood or brass. For one form of test he traces the pattern with the intent to determine its shape, which he is then asked to draw on a sheet of paper, his eyes still being closed. In other forms he is asked to find and draw perfectly the true pathway of a maze. The maze may then be rotated through 45° , 90° or 270° and the effects noticed. Or the patterns may be traced over the head or with the hands behind the back. Introspections are required in every case.

These tracery tests are frequently much more successful in eliciting visual imagery than kinaesthetic, but with certain persons they bring out the kinaesthetic element with great distinctness.

Another form of motor imagery, or at least another way of using it, must be recognized and provided for in any adequate tests although it is introspectively very difficult to identify and only practiced observers are likely to detect it. I refer to what Professor Colvin, in a recent very interesting paper¹ has termed 'mimetic imagery,' not altogether happily, as it seems to me, for although the imagery is sometimes mimetic, and possibly it was all originally so, it is often purely symbolic. I should prefer the term 'symbolic motor imagery.' I have had one marked case under observation more or less directly for upwards of twenty years. The matter is referred to in my *Psychology*, but in a manner which might well escape attention.

¹ *Psych. Bull.*, vol. vi., 1910, p. 223.

Individuals using this form of imagery carry on their reflective thinking in terms of schematic and often faint motor imagery which they use in as purely symbolic a way as that in which verbal thinkers use their words.

No special tests have been devised so far as I know to bring out this type of imagery, and I have none to suggest. If the subject of the test is introspectively acute, almost any of the tests heretofore described will serve the purpose, particularly perhaps those dealing with the solution of simple problems. If this introspective alertness is wanting, I know of no device which will materially assist. None of the ordinary distraction and help tests promises to be of any substantial assistance, because the imagery employed carries a symbolic import often entirely remote from any peripheral factor that can be introduced. It is accordingly all but impossible to impede the flow of the imagery except by such distractions as obliterate the attention process itself; and aids are equally impracticable because one cannot anticipate and so 'hitch on to' the reigning symbolism.

M. Cutaneous Imagery. The main fields for the collection of evidence dealing with this group of images, apart from the questionnaire, have been the tests on sensory discrimination, as in the experiments on touch and temperature, those on localization and a few on memory. They seem to indicate in general, that imagery of the several cutaneous varieties can be detected by most persons under favorable conditions, that with not a few individuals it is very vivid, that it is subject to confusion of a very baffling kind with the corresponding sensations, and finally, that many people make little or no use of it if other imagery can function for it, as is generally the case, particularly where, as in localization processes, visual images are available.

N. Organic Imagery. Introspection, which is practically the only mode of approach to organic images, is peculiarly difficult for two reasons: (1) We have little practical necessity to attend to imagery of this variety and we may well suppose that lack of practice is as significant here as it is elsewhere. (2) Much more important, however, is the well

recognized fact that when we attempt to call up an organic image we are likely to stimulate reflexly motor and circulatory changes in the part of the body thought of, and forthwith, we have sensations blending with or displacing our images. So much is this the case, that not a few trustworthy psychologists deny the reality of organic images just as they have denied the existence of kinaesthetic images.

O. Tests on Gustatory and Olfactory Imagery. Little or nothing has been successfully achieved here to work out a special technique, and our information rests again mainly on (1) ordinary introspection, (2) sensory discrimination tests, and (3) memory tests. The practical difficulties are, if possible, even more serious than in the case of the previous group. In the gustatory process, introspection is handicapped because the thought of tastes almost inevitably excites reflexly the glands and muscles of the mouth, so that sensations are at once aroused to fuse with or displace the image, assuming that the image is really available. Similarly, in smell, there are reflex excitations of the inspiratory muscles leading to illusory sensations.

The most trustworthy experimental work on the memory of these sensations indicates what theoretically, and on the basis of introspection has generally been held, *i.e.*, that very few persons can readily get genuine olfactory images. This fact is a trifle paradoxical in view of the extreme ease with which olfactory illusions can be produced, a circumstance which would apparently indicate a high degree of central olfactory excitability. Taste-imagery is perhaps more frequent, but the images are probably fusions of several distinct properties as are the corresponding sensations, and they rarely occur free of combination with the sensory activities for the reasons indicated above.

PART II. RECOMMENDATIONS.

After this brief resumé of available methods we may proceed to present certain recommendations for those who propose to use imagery tests. But first it must be made clear

what conditions such recommendations are designed to meet.

There are several distinct aims which have been more or less well differentiated in the previous investigations of imagery.

Tests have been sought by psychologists which might be used with groups of persons in an objective way and especially has this been true of the search for methods to use with children in the schools. Other investigators have been mainly interested in the intensive analysis of individuals and especially in the search for 'types.' They have sought (1) to determine all the forms of imagery which an individual can command at will. They have sought (2) to ascertain the forms actually employed in the common mental processes of daily life. This latter undertaking may be so executed as to determine not merely *what* imagery is thus used, but also *how* it is used as regards the distribution of the several varieties of it among different kinds of mental activities. In more detailed execution of both these two main aims, attempts have generally been made to grade the imagery discovered with reference to one or more of its ordinarily recognized characteristics like intensity, stability, profusion, accuracy, ease of attainment and the like.

Investigations have also been made with a view to determining the function of imagery. In this connection we meet the experimental literature of imageless thought.

No one of these aims can be realized by any single quickly executed test and one must squarely face the alternative of abandoning any pretense of accurate information concerning the imagery of a given individual, or else of undertaking a rather extended group of tests, whose outcome may be finally problematic if the subject proves too deficient in introspective powers.

One will be well advised who enters upon such analyses without expecting to come at once upon any of the conventionally accepted 'types.' Certainly the visual and auditory types of Charcot and Galton will rarely be recognized in any adequate tests, and even the more recent distinction of word-types and object-types will be found extremely elusive in

many cases. The shifting and substituting of one form of imagery for another under slight changes of conditions, where no one could possibly have suspected the occurrence of such a transformation, speedily render the unprejudiced observer skeptical of all rigid divisions into types of the familiar kind. This is not to deny the reality of types, but simple to urge that they do not follow with any great regularity the lines heretofore laid down. They represent problems we still have to solve, rather than solid foundations on which we can build. A subject who uses visual-object imagery almost wholly in one class of tests may be equally wedded to auditory-motor word imagery in another type. My own observations have abundantly convinced me of this, and confirmation exists in plenty in the extant literature of the subject. No doubt it often occurs that imagery of some one sensory mode dominates markedly over others, but the predominance is probably in many cases much less than has been generally assumed since the work of Charcot and Galton, and it is certainly much more difficult to establish the facts than has been commonly recognized.

As far as possible it seems desirable to make a considerable number of the tests used comparable in character with ordinary mental occupations, so that the results may have a closer bearing on normal conditions. This may be urged while still recognizing that such tests need to be supplemented by others designed to sound possibilities rather than merely to determine norms. We must admit the possibility that even our tests intended to simulate ordinary tasks and familiar situations may be abortive simply because they are known to be experiments on which introspection will subsequently be demanded. The failure of all purely objective tests robs us of any power wholly to escape this possible difficulty. No doubt this element enters in to disturb certain results. We have mentioned above cases where the knowledge that introspections were to be asked has radically transformed the character of the imagery. However, it has not been because the subjects were attempting to take at one and the same time two contradictory attitudes, one of absorbed learning and one

of watching the learning, but simply because the attitude of memorizing with the expectation of being subsequently tested proves to be different from the attitude of concentrated attention without thought of future recall. The difficulty must be recognized and as far as possible overcome. It is in no way fatal and is one familiar to every experimentalist.

We shall recommend two groups of tests containing in part identical members. One of these will be designed to furnish a *brief* survey of the imagery capacities of the subject and the other will offer a more *intensive* and more *accurate* analysis. The tests selected are believed to possess points of superiority in each case justifying their selection in preference to other available ones.

On the negative side it is to be clearly understood that in this report we are not primarily engaged upon the problem of the function of imagery. It is also to be understood that the defects of all exclusively objective methods debar us from recommending any series of tests as conspicuously and unambiguously suited to testing groups of adults or children.

For the briefer series we recommend as described below the questionnaire, the test on melodies, the 'description' test, the spelling test, the reasoning test and the test on writing. This provides for a cursory examination at least of voluntary imagination, of auditory and visual objective memory, of auditory, motor and visual verbal memory, of reasoning and of one form of voluntary control. Save for the questionnaire, such a group of tests skillfully conducted need not occupy over two hours, and the questionnaire can be written out at leisure. The order in which the tests is given is not essential, but the order above represented is advantageous as it places the reasoning test and the writing test last. These are apt to be rather more difficult than the others to secure introspections from.

The recommended tests are numbered as follows: Part II—I-A, II-D (1), II-E, II-G, III-M, IV-N.

For the more extended series we recommend, in the form described below, the questionnaire as a test on voluntary imagination; an association word test and a test on verbal

similarities as testing both spontaneous and voluntary verbal imagery. If one wishes for the sake of completeness to examine spontaneous imagery of objects, this can best be done by taking care to include in the questionnaire used a series of inquiries about the train of images in reverie and when falling asleep. We regard this as unnecessary, because the essential facts are almost certain to appear in connection with other tests to be mentioned, especially the descriptive tests.

In the range of memory we recommend for the survey of auditory imagery a test on single tones, one on melody and harmony, and a test on noise. The tests on words occur in connection with a later group. For the analysis primarily of visual memories the test on the description of objects and the description of pictures, colored and in black and white, are recommended. These will care for objective-visual imagery. The letter-square test and the spelling will bring out the verbal material and the first may be used for forms and numerals also. The test on reading should be added. It belongs in part to the analysis of perception, in part to volition and in part to memory. It sometimes produces little or no material other than verbal; sometimes it succeeds in calling forth profuse objective imagery. In any case it represents a highly important and familiar mental operation which should not be omitted from examination.

Kinaesthetic memory imagery (?) is certain to be elicited from many subjects by the tests on melody, on words and on the specific voluntary acts like writing. It can be further examined in the tests on estimating extent of movement, and on following tracery patterns.

Cutaneous memory imagery may be tested by experiments on localization.

No special tests on taste are recommended, because the results of all those available are too ambiguous. The questionnaire replies represent about as high an order of accuracy as we can secure at this point.

A single test on olfactory memory is recommended more by way of completeness than because its outcome is likely

to be very important. This is in substance Miss Gamble's test on reconstruction.

On reasoning processes we recommend problems in arithmetic and geometry as described and at least one problem of a social or ethical character, and one of a purely practical character.

Tests on voluntary control may be carried out to great advantage in any learning process and an extensive literature is at hand for guidance in such tests. We recommend only the tests on writing, (1) because the consumption of time in the other tests is prohibitive for most persons and (2) because, with the exception of the cases where a new and unused muscle group is coming under control, we believe the writing control is essentially typical of all the ordinary motor activities under conscious direction.

Such a group of tests as this intelligently applied, with the minor variations which may be required by the peculiarities of a given subject, will result not only in affording a highly satisfactory inventory of the individual's equipment of imagery, it will also convey a very significant impression of the actual distribution of his imagery and of the manner in which he uses it.

We pass now to a more explicit and detailed account of the tests selected, together with a brief commentary upon their characteristics.

In order to make sure that the tests chosen cover the ground fairly each of the main forms of mental activity involving imagery is herewith examined. For practical purposes this may be done by examining the play of imagination under various conditions, (this includes the process of apprehending meaning as in listening to reading or talking); the use of memory, both mediate and immediate; the process of reasoning and the execution of acts of voluntary control. The tests recommended are grouped on this basis, although other principles of classification might be equally useful. Apology may be made once and for all for the inevitable repetition in the next section of points touched on in the first part of this report.

DIVISION I.

Imagination.

A. Questionary. Although as commonly employed, the questionnaire involves a measure of distinctively memory material, as a whole the method may be regarded as one appealing primarily to the voluntary use of imagination. It is, moreover, the best available general test on this mental process and may with rare exceptions be advantageously used.¹

Several questionnaires are available of which Galton's, Titchener's, Seashore's and Betts' may be mentioned.² These may serve as patterns. The last three possess advantages over Galton's, but his has been so frequently used that there is a good deal of interest in comparing one's results gained by means of it with those previously reported. The main considerations to be met by such a document are clarity of questions, recognition of every genus of images and each important species, and care not to over-emphasize the commoner varieties such as visual and auditory; suggestions for evaluating on some arbitrary scale differences in such characteristics as vividness, accuracy, detail, permanency, and ease of control of the various images. The length of the document and the refinement of questions into which it goes are matters to be decided by the amount of time available.

B. Association Test. Certain of the tests on association are fairly to be regarded as tests of imagination and one of these we recommend because of its simplicity and ease of application and also because, as contrasted with the questionnaire, it brings out spontaneous in distinction from voluntary imagery. It involves on the whole a more natural type of mental act than the questionnaire. With some subjects the test is likely to prove merely a test of verbal imagination, but even so it is worth making.

Two lists of words should be prepared, the number to be

¹ It will be remembered perhaps that we have rejected such tests as the ink-blot test, which undoubtedly have an interesting bearing on certain forms of constructive capacity, because they do not bring out imagery with sufficient certainty.

² Galton, *Inquiries into Human Faculty*, Appendix E; Titchener, *Experimental Psychology*; Student's *Manual*, Qualitative, 198; Seashore, *Elementary Experiments in Psychology*, 108; Betts, *Distribution and Function of Mental Imagery*, p. 20.

determined by the time available (twenty has proved a convenient number in my own tests and those of others), but not less in number than will allow some appeal to each important type of imagery.¹ One of these lists is to be read visually, a word at a time, by the subject, the other is to be read to him orally. After each word he is to write down the idea or word suggested by the stimulus word, describing the imagery in which the associates are presented.

Sometimes the test is made by confining the reaction to this associated idea, sometimes the associative train is allowed to run on for a period—five or ten seconds or more. The series is at once recorded and introspections are given. Other complications such as distraction can be introduced. With unpracticed subjects the briefer procedure is preferable. With practiced introspectionists the results of the longer trains are more suggestive, although difficult to tabulate or quantify satisfactorily.

C. Sensory Similarity. We recommend a test which lies on the border between imagination and memory involving in part spontaneous and in part voluntary reactions. This is the test on sensory similarity. The subject is given a word *e.g.*, 'cat' and told to write all the words he can which rhyme with it. He is then instructed to write all the words he can think of ending in a given series of letters, *e.g.*, 'ous' without regard to their pronunciation. A limited time, five minutes perhaps, is set, and introspections are asked. Several experiments may profitably be made with different word-stimuli. The test is even more purely verbal than the previous one, but in practice the rhyming form has been found to be peculiarly useful in throwing into the foreground of consciousness auditory and motor speech imagery, not so readily observed in the preceding tests. The other form appeals strongly to visual imagery. Like all these tests it can be complicated indefinitely by distractions and other devices.

¹ The experiences in the Chicago Laboratory would not indicate that any particular words chosen possessed permanent merit over others. The list published by Secor (*Amer. Journ. of Psych.*, vol. xi, p. 225 ff.) may serve as an example, although it tends to slight possible taste and smell imagery. Such words as sky, whistle, violets, sugar, ice may illustrate the materials.

DIVISION II.

Memory.

To be in any way complete as an account of the use made of imagery in memory processes, involves tests covering the various sense departments recognizing the difference between mediate and immediate memory, and the differences dependent on the condition during learning, *e.g.*, whether the subject knows he is to be examined on his knowledge. The tests will be described without invariable reference to these qualifications, but it must be clearly understood that they concern factors which distinctly affect imagery. They must be taken into account in any complete series of tests.

D. Auditory Tests,—Tone. Two types of tests on tone imagery are available, one involving single tones, or groups of tones experienced simultaneously, the other involving melody, thus bringing in rhythm and if desired, harmony. The second can be complicated indefinitely by adding harmonization in terms of a single instrument like the piano, or of many instruments as in an orchestra. The phonograph can be used for this purpose. With persons richly endowed with auditory imagery the more complex tests are sure to be worth while in disclosing intricacies in the organization of imagery not otherwise revealed.

(1) The test can be made with any musical instruments but a familiar instrument like the piano is preferable, as strangeness of tone quality is a distraction to untrained observers, and even with trained observers, the familiar tones are more easily and confidently reproduced. If brevity of time must be consulted, the test on single tones may better be omitted than that on melody. The writer has never chanced to find a case where auditory imagery was gotten with one test and not with the other; but the relative vividness of the imagery occasioned by the two tests varies with different individuals, so that on a scale to show this feature the tests would give contradictory results. The melody test simulates common experience more closely than the other and is easier for untrained subjects to perform.

The subject is told that the first few notes of a familiar melody will be played, enough so that he can recognize it. He is then to finish the melody mentally—'in his head.' Then he is asked to describe the experience. Tone quality, pitch and tempo should be noted and the melody may then be actually played through again to see how it compares with the subject's recollection of it. If possible, the melodies should be played at three points on the tone scale, namely in the deep bass, in the high treble and in the middle register.

The test has two disadvantages, (1) with persons possessing weak auditory, but strong vocal kinaesthetic imagery, it tends to the submerging of the auditory in the kinaesthetic. (2) If the melody has familiar words set to it, persons of strong verbal imagery may again tend to swamp the auditory factors with enunciatory elements. The chief merit of the test is that it 'works itself' almost without instructions, provided the subject has any musical imagery at all.

(2) The test of single tones is conducted as follows: A series of at least five tones is chosen forming a chromatic series, no successive notes to be more than a half tone apart. The piano is again the best instrument, but the tuning fork or organ pipes of the ordinary laboratory equipment will serve.

A tone is struck by the experimenter, the subject then counts silently 'by threes' for five seconds (to break up the carrying over of the after-sensations, or the suppressed humming of the tone). Then he is asked if he can hear the tone mentally. If he replies 'yes,' the operator strikes one of the closely related tones and asks, 'Is it this;' striking several notes, one after the other, including the original tone. This procedure not only stimulates a subject to try to form a correct image, but it also assists him to observe peculiarities in the image, whether it is relatively faint or vivid, whether it is flat or sharp as compared with the original, whether the timbre is correct, or is, as often happens, the tone quality of the subject's own voice.

(3) Noise. Tests on the auditory imagery for noise have less practical importance and for diagnostic purposes

are only essential provided the questionnaire has given negative results, although if one desires to ascertain with some approach to completeness the distribution of imagery forms, such a test as follows would be needed.

A test may be made following the lines of that on tones. Homely devices may be pressed into service. A large empty box, a small cigar box, a table top, a chair back, and a wooden rod, may serve as equipment. Other similar utensils will suggest themselves. The first four articles are to be smartly struck one at a time with the rod; the subject is to count again as in the tone test and then try whether he can get an image of the noise. The experimenter may also crush in his hand a stiff piece of paper using a single quick motion for the purpose. He may also make a sharp hissing sound and test the subject's imagery as above for the two kinds of stimuli.

(4) Numerals and Words. Special tests on the memory of numerals and words from auditory stimulation are not indispensable in view of the tests to follow, provided one is seeking simply for the existence of imagery rather than its distribution. But if for any reason one feels the need of tests isolating these factors more completely, the following is recommended:

The old dictation test used so often for investigating the memory span. Lists of digits are prepared and read distinctly to the subject in groups of five, six, seven or more at a time, thus—7, 4, 3, 6, 9, 8,—5, 2, 7, 4, 9, 1, 3. The subject is at once to write down or pronounce the numbers as remembered. The writing is likely to emphasize visual imagery in persons who can use it freely, especially if done with closed eyes. The pronouncing is likely to emphasize the auditory vocal elements. The lists may be read partly without rhythm, and partly with it. The recall will generally reflect the effects of this variation. An interruption of a few seconds between the stimulus and the reproduction, as in the test on tone, will naturally decrease accuracy of memory, but it is quite likely to assist the introspective identification of the imagery.

Nonsense syllables and lists of disconnected words may be similarly dictated. Mediate memory for such materials may obviously be tested by learning the lists by heart and reproducing after an interval of time.

E. Visual Description Tests. Of the visual tests we recommend the following which in practice elicit many forms of imagery other than visual, but which are particularly successful in stimulating the latter. They belong to the general class of description tests.

A test for objective visual memory may profitably be used which is based on an old game. In a tray or on a table are placed at least ten or fifteen small and familiar objects—coin, thimble, penknife, ring, and the like. Some of these should be distinctly colored, *e.g.*, a red pen-wiper, a blue ribbon and a yellow pencil. The subject is allowed to inspect the articles for a specified time—about one second of time for each article has been found judicious—and he is thereupon required to write down or name all the articles he can remember. His introspections are then taken, and questions are asked about the color, size and relative location of the objects. An excellent test proceeding in a similar manner and resembling the well-known 'Aussage' tests, involves the use of drawings, part of which should be colored and part in black and white.¹ A set of ten, five of each, will serve the purpose. These may be used either as a test for immediate recall or for recall after a few minutes, hours or days.² In either case the subject is asked to describe the picture with detail, as in the preceding tests and then tell how he executes the task, grading his imagery if intensive classification is at stake.

¹ Dr. Fernald has used successfully prints of the picture postal card size. She has found the small size which enables the eye easily to get the whole scene at a glance preferable to larger pictures where attention is scattered more widely over the field.

² Although the imagery changes in the most amazing fashion as is well known, the author has found no reason to think that commonly there is a change in memory depending primarily on time, from one sense quality such as vision to another such as audition. One may, under pressure of experimental direction, use in immediate memory a form of imagery not ordinarily employed, *e.g.*, tactual; but if the immediate recall would normally be in visual terms, the recall at a later period is not likely to be other than visual. Other investigators have confirmed this. Cf. Segal, *Arch. f. d. ges. Psychol.* 1908, vol., xii, p. 124.

Questions containing erroneous suggestions as to what he has seen bring out most interesting results and should be followed by showing the picture a second time.

Where the test is used merely to ascertain the presence of imagery, no further precautions are necessary, and it may be safely laid down that persons who secure no visual imagery from such a test are, practically speaking, lacking in it altogether. If the tests are used as a method of ascertaining the distribution of imagery in normal processes, a sharp distinction must be drawn between the cases in which the subjects know that they are to be held for a report and those in which they are unaware of this. A relatively naïf inspection of the card has been attained by Dr. Fernald through the use of 'puzzle pictures,' which the subject inspects merely to find the concealed objects. This gains a good degree of attention without throwing the subject into the learning attitude. But the same result can be gained in many other ways. The pictures may, for instance be shown with a view to passing on their relative aesthetic excellence.

F. Letter Square Test. Blanks are prepared with nine squares arranged to form a larger square. (My own have been 1 inch on the side, making a large square 3 inches on a side.) In these squares can be placed numbers, pictures, forms, letters, syllables, or words. The subject reads through the list three times and then attempts to reproduce it by writing in the spaces of a blank form, or he may reproduce the materials verbally. Introspections are then asked. The test may be usefully and easily complicated in several ways; by reading (when the material permits) the lists aloud to the subject after which he is to put the remembered elements in appropriate order on a blank square. Distractions, auditory, vocal-motor, or visual, may be introduced as heretofore described. Rhythm may be introduced, the subject may be instructed to think of the material in groups and so on.

This test is one of the best in the list for bringing out verbal imagery, both visual and auditory-motor. It also brings out certain forms of the objective-visual, but it is on the whole

much less satisfactory for producing objective imagery than the description tests. If the auditory dictation is employed, it would rarely be worth while to use the previous test on numerals and words, number II-D4. Both overlap in some degree the following test on spelling, a test which ought in no event to be omitted.

G. Spelling Tests. In part of the tests the subject should write the word to be spelled, in part pronounce the letters. In spelling forward, words should be selected which will put some tax on the subject's capacity—shorter and more familiar words can be used with children, longer and more uncommon words being selected for adults. With the common and well known words the spelling will often be found so highly automatized as to involve little or no imagery.

Spelling backwards is even more useful than spelling forward. The test can be made in two ways: (1) The subject is given a word, *e.g.*, 'locality,' and then pronounces or writes—better the first—the letters of the word backward. (2) The experimenter pronounces the letters backward and the subject attempts to determine the word and pronounce it himself. The control of the rate at which the letters are read will materially effect the success of this form of the test. Different subjects vary widely in their response to the test and the rate must be adjusted to their individual capacity. It should be rapid enough to keep the attention strained, but not so rapid as to produce a serious confusion. For most subjects the rate of one letter per second is a good one to begin with. The subject gives his introspection at the end of each test. The test can be complicated with the methods of disturbance and help.

H. Reading Tests. Reading affords an opportunity for various forms of tests. They should be included in any series designed to afford a survey of the imagery in normal types of mental action; but they present introspective difficulties which often render them of secondary value for use with untrained observers and especially children.

The passages selected may be determined in part by local conditions. As a rule they should not exceed ten or fifteen

lines in length and may well be shorter. Both prose and poetry should be represented, and among the poems should be representatives of well-known songs (to stimulate auditory-vocal imagery). They may advantageously include (1) passages descriptive of nature in her appeals to the various senses, (2) passages describing some concrete practical operation, *e.g.*, the planting of trees, the making of brick, and the like, (3) passages discussing some distinctly abstract ideas, whether of a philosophical or a scientific character.

The material should be presented (1) visually and (2) orally. It should be given (a) under conditions where the subject knows that he will *not* be examined on the contents of the passages. He is simply asked to read or listen as he would in the case of any matter momentarily attracting his interest: (b) contrasted with this procedure, the test should be made when the subject knows he is to be asked to give the import of the passage. This he may either write or give orally, the former generally proving in practice the more useful. He may be asked for the general import of the passages or for as much as he can recall verbatim. He may react at once or after an interval of time. His introspections are asked both on the process of apprehension and of reproduction. The passages read for introspection on the process of apprehension are not to be used in the tests on retention. Various complications will suggest themselves.

I. Cutaneous Tests. For contact stimulations the subject (his eyes being closed or averted) may be touched lightly on the forearm with a blunt cork point. He then counts aloud by 'threes,' for ten seconds, to allow any after-sensations to subside. He then attempts to recall the sensation and points at the spot stimulated. His introspections are then requested.

With blunt metal rods a similar test may be made for warmth and cold, the rods being kept in hot or cold water and carefully wiped before being applied to the skin. The points should feel to the subject's skin distinctly cool and distinctly warm, but not smarting nor painful when left on a few seconds. The after-sensation must be carefully avoided.

The stimulations should in no case exceed a second in duration. The counting should go on for at least ten seconds, and twenty is better. The subject is then invited to recall the sensation if possible and point to the spot where it was located.

Pain may be tested by a light needle prick or by snapping a rubber band on the back of the hand. A considerable interval of distraction must be used, the length depending on the violence of the stimulus. The experimenter must determine himself by control tests how long to allow for the subsidence of the after-sensation. Otherwise the procedure is as with temperature sensations.

These tests fall out very differently with different persons, some subjects failing to get the images with any certainty, others getting them easily, and still others being flooded with visual imagery of the parts stimulated. But it may be fairly assumed that a subject who under these conditions gets no trace of cutaneous imagery, never makes any important use of it.

Interesting experiments involving imagery from fusions of touch and motor elements may be made by using the raised letters of the alphabet for the blind. The subject is aside from our present main purpose and need not be discussed.

J. Kinaesthetic Tests. Cut out of wood, metal or cardboard designs into which a pencil point will just easily fit. The designs may be geometrical figures, *e.g.*, triangles, squares, oblong figures, circles, ellipses, etc., or they may be continuous line letters like 'S,' 'Z,' 'O,' 'M,' or 'V.' We have used in the Chicago Laboratory a figure like the maze of the comparative psychologists, but other purposes were primarily in view and for untrained observers simpler figures are better.

With eyes closed or screened the subject traces the figures with a pencil in the slot, the operator putting the pencil at the start at one or other extremity of the figure. The tracing may be done one or more times depending on the complexity of the figure. Then the subject is given a blank sheet of paper in place of the pattern and on this he is to report the design as he remembers it. His introspections are then asked. Half a dozen tests with different designs are sufficient.

An interesting variation of the test, often resulting, as does the original, in throwing more light on visual than on kin-aesthetic imagery, consists in having the subject attempt to reproduce the figure as it would appear if turned about through 45° , 90° , or 270° . We have gotten many striking results, too, from attempting either to interpret or to reproduce figures traced in unusual positions, *e.g.*, behind the back or above the head.

A simpler test, but on the whole less successful, is the familiar estimation of linear magnitudes. The subject sits facing a table on which is a horizontal wooden or metal rod supported by clamps attached to upright rods. Two stops are attached to the horizontal bar at a convenient distance apart, say 25 centimeters. The index finger of either hand measures off the distance between the stops by sliding along the bar. The second stop is then removed and the subject attempts to measure off the original distance. The eyes must be closed or averted. If short distances are used, the finger may move freely in the air between stops which must be higher for this purpose. The object of this procedure is to eliminate the contact sensations. Introspections are then recorded.

K. Taste and Smell. In the case of olfactory and gustatory imagery the author is disposed to recommend going beyond the questionnaire only when the results from that are negative or highly ambiguous, or when a complete census on distribution is desired. The difficulty of distinguishing between sensations and images is similar to the difficulty with organic imagery and perhaps quite as serious. We have however in the case of smell, at least, available tests which may assist by stimulating introspection.

One of Miss Gamble's tests may serve to meet the olfactory case. A series of ten odorous substances is prepared in shallow wide-mouthed bottles, with glass stoppers.¹ The subject sitting blindfolded takes the bottles in the order

¹ The following list is easily secured and will serve the purpose. The material in any psychological laboratory will afford other equally good groups. Anise, benzoin, bergamot, cassia, cloves, camphor, creosote, musk, peppermint, sandalwood.

in which they are presented, removes the cork, inhales once and replacing the stopper, turns to the next bottle. After the series has been twice thus presented, the subject is given the bottles in disarranged order and asked to arrange them as first experienced. The test can be repeated several times with the same materials by changing the order of the stimuli.

If olfactory imagery is available, it is reasonably certain to appear under conditions such as these. The best statistics make it seem probable that such imagery is relatively rare. This conclusion is at variance with the outcome of many questionnaire investigations. I have frequently, when using the questionnaire, received affirmative answers, as have other investigators, to the inquiries about such images. I am disposed to attribute the discrepancy to a failure on the part of more naïf observers to discriminate between pure olfactory images and mental states which are either illusory sensations or fusions of peripheral with centrally aroused factors. The more careful experimentation tends to render subjects more sensitive to this distinction and more capable of avoiding the possible confusion involved.

No experimental technique seems to the author successful in so controlling the glandular and muscular activities of the mouth as to make certain that sensations and images of taste are kept apart. The ordinary laboratory forms of stimulation in connection with sensory discrimination, for example, aggravate rather than help this difficulty. At present, therefore, the writer is not prepared to urge any tests applicable in a general way as being materially better than the results from the questionnaire, uncertain and unsatisfactory as these are.

L. Organic Tests. Owing to the serious difficulty of distinguishing organic sensation from organic imagery it does not seem profitable at the present time to go beyond the inquiry of the questionnaire, or the possibilities of ordinary introspections.

DIVISION III.

M. Reasoning Tests. A few tests on reasoning processes ought to be included in any diagnostic series, not only for completeness in a program aimed at determining the distribution of imagery, but also because they serve to bring out under quite natural conditions forms of imagery essential to the thought processes of the subject.

Among the most practicable are simple problems in arithmetical computation. For instance, the experimenter may dictate two-place numbers, to be added or subtracted mentally. Three, four and five place numbers may be written down and the arithmetical operations carried on while looking at the paper, but without the assistance of pen or pencil. Introspections are asked for after the subject completes and announces the result of his work.¹

To test the geometrical processes the problem of the cube mentioned on page 76 may be used or one of this kind: The subject is told to think of a square; if a line is made joining a pair of diagonally opposite corners, what shapes have the resulting figures? If the other two corners are similarly joined, what shapes have the resulting figures and how many are there? Are they alike in shape and size? 'How do you know?'

Simple problems may be selected from any range of practical, political, or scientific interest, the only essential conditions are that the question shall be lucid and the answer within the capacity of the subject to reach. For this purpose a problem so difficult as simply to baffle attack is futile.

Something of this kind may serve as a practical problem: What route would you take and how would you go about making arrangements, if you were to travel from your home to Siberia? Give the steps in as much detail as possible. Many other more immediately personal problems will suggest themselves and may profitably be used.

Problems dealing with concrete ethical situations may

¹ Dr. Fernald has tried having the computation carried on in terms of the Roman alphabet numerals, but without eliciting any forms of imagery not otherwise accessible.

bring out interesting results, although the introspection required is rather too difficult for untrained subjects. Such questions may be illustrated by the old ethical problem whether it is ever right to lie. In the case of a dying man who cannot possibly recover, is it not justifiable to lessen the pain of his final hours by concealing from him the seriousness of his condition? Questions of this type and problems dealing with relatively abstract relations, may bring out interesting evidence concerning the conceptual imagery employed. It may be mentioned in this connection that the recent studies of the process of judging, afford a large range of tests in which the analysis of imagery may be carried out.¹

The experimenter ought to be especially alive in tests of this general variety to the presence of symbolic motor-imagery, and enough non-mathematical problems should be given to make sure whether or not one has this kind of imagery in use. We bar mathematical problems because vocal-motor, or hand-motor imagery referring directly to numbers, is likely to be used in such cases.

DIVISION IV.

N. Voluntary Control Tests. Writing affords a very easy and effective means of access to imagery as employed in the control of voluntary movements. Like spelling it is in part a memory test, but in the forms to be mentioned here it is primarily a test on imagery in its relations to voluntary action, and it furnishes, together with the tests on spelling and reading, a transition from experiments on mere memory to those concerned with the analysis of voluntary action. [Spelling and reading have been treated under memory.]

The subject may be asked to write either from oral dictation or from the printed copy. His introspection may then be obtained and the results compared with a repetition of the test in which fresh material is used. A paragraph containing a hundred words will serve the purpose. It is well to select one in which the vocabulary is not too familiar and common-

¹ Cf. Titchener, *Experimental Psychology of Thought Process*, New York, 1909.

place. Passages from text-books on philosophy and psychology fill the bill admirably for ordinary subjects. In at least one set of tests the material written should be something known by heart, a verse perhaps. The writing begins at a given signal and on completion introspections are at once asked.

Writing the letters of the words backwards [the words being spelled either forward or backward] from oral dictation, writing every other letter of the word, writing left-handed, writing upside-down, and looking-glass writing, all are excellent devices for throwing imagery into the foreground, and all but the two last are done with sufficient readiness by most subjects. Full introspections are in every case required.

The entire series selected may then be repeated with the eyes closed. This is often a strikingly useful variation of the test.

We have found it possible to introduce the distraction methods quite successfully into the test. For auditory distraction one may listen to a metronome, for motor distraction one may repeat some well-known verse, either aloud or under the breath, or one may count rapidly. For visual distraction one may let the eyes follow the contour of some conventional pattern, or trace the lines of overlapping spirals. (The difficulties in securing satisfactory distractions have already been mentioned.) The procedure in the case of auditory-motor distraction may suffice to make clear the method. The subject with eyes closed is told upon receiving a signal to begin writing the first verse of 'America;' while he writes, he is to sound the vowel 'a.'

A great many experiments have been conducted in recent years, to determine the psychological processes controlling voluntary action, and especially with a view to determining the part played by imagery. If the tests on writing and spelling are used, especially in the more infrequent forms like writing with the left hand, and like spelling backward, there need be very few additions, at least so far as concerns ascertaining the kinds of imagery which can be commanded. If exhaustive tests on normal distribution are desired, we must

add to the list tests involving new coördinations, or at all events, radically new combinations of old coördinations. Learning (1) to move the ears, to move the scalp over the crown, to move the toes independently of one another, and (2) learning to finger a musical instrument may illustrate the two main types of cases.¹

Tests of this character require so extended a period of work as to render them rather inappropriate for detailed mention in context with the other tests of this group. Reference is made below to one or two studies on the learning of such acts. An investigation of one form of the process is now nearing completion in the Chicago Laboratory.²

PART III. GRADATION OF IMAGES.

Most of the investigations hitherto conducted have undertaken to offer some quantifying statements with reference to the vividness, stability, ease of attaining the images, and so on. In the opinion of the present writer the gradations which have often been employed in such scaling of the qualities of imagery, are too refined to be successfully employed by the ordinary observer. For instance, writers on imagery who report upon grades of intensity or grades of vividness³ have often employed six or more distinctions in accordance with which they have asked their subjects to make their reports.

The effort to subject this matter to a satisfactory series of control tests proved impracticable for the following reasons.

¹ The case of learning to move the ears and learning to move the toes independently of one another differ in that one involves gaining control over a wholly unused muscle, the other involves separating muscular innervations which have always occurred together.

² Bair, Acquirement of Voluntary Control, *Psychol. Rev.*, vol. viii, 1901, p. 474. Book, Psychology of Skill, *University of Montana Studies*, vol. i, 1908.

³ The author does not feel under obligation to enter upon the controversy concerning the genuineness of the intensive attribute as applied to imagery, nor the differentiation of intensity from vividness and of both from clearness. If vividness or intensity are either of them genuine characteristics of the image process, gradation is at least theoretically possible and it becomes proper for a report of this kind to make recommendations concerning it. That ordinary observers are able to make distinctions which seem to them natural and easy, when asked to grade their imagery in this way, affords a strong *prima facie* case for the genuineness of the distinction reported.

If a series of images could be secured on a number of successive occasions, with precisely the same degree of vividness, it would be possible by tabulating the classifications made by the subjects, to ascertain with what degree of nicety they could consistently grade the experiences. But evidently we can secure no such guaranty of the actual similarity in vividness of the imagery on these successive occasions. Any consistency which the subject may evince becomes therefore ambiguous and inconclusive. On the other hand a failure to classify the images in the same way on different occasions is no guaranty at all that the classifications are untrustworthy.

The author's observations extending over a number of years and including in their range a large number of individuals leads him to believe that occasionally subjects are able to grade their images on the scale of eight with entire confidence and with practical consistency. The difficulty which is most likely to be troublesome in the case of such observers is the reduction to a common scale of imagery belonging to two different sense departments like vision and audition. The maximal degree of vividness in the one case may seem very different from the maximal degree in the other, and still the subject may feel indisposed to call the weaker group of images in any sense faint or indistinct. Our statistics do not at present in the author's judgment afford us adequate information as to whether the scale of intensities for the different forms of imagery is of equal extent, much less whether the threshold of noticeable differences in it is any wise homogeneous. Under these circumstances one must regard any estimates of gradation as being merely rough approximations.

For the reasons just discussed, as well as for another highly important reason next to be mentioned, the author has come to believe that three gradations are as much as can be profitably and safely recognized in the presentation of statistics on imagery. The additional reason which has had great weight with the author has been the testimony of several psychologists of distinction and wide experience that they themselves found it entirely impossible to make so many gradations as the questionnaire commonly demands. The procedure here

recommended would in the case of vividness, for example, recognize three divisions which may be called very vivid, moderately vivid, and faint. Subjects who feel the necessity for intermediate grades can be supplied with these by the use of plus and minus signs. Thus, the gradation faint might be modified to provide two more classes by adding a plus sign for images which are slightly above this level and a minus sign for those which are below. A similar treatment of each of the other main divisions would give nine differentiations, which is fully adequate to care for any case that the author has ever encountered.

Even after we have agreed upon the number of gradations to be recognized, the problem still remains in dealing with individual subjects, how we are to instruct them in the use of such standards, which are in the nature of the case rather arbitrary and altogether subjective. On this point the author has no dogmatic advice to offer. In his own experience two types of procedure have seemed practicable. In one, the subject is given no definite advice or suggestions, but is simply advised to wait until he has inspected twenty or thirty images before beginning his gradations. He is, moreover, allowed to revise his gradations if they appear to him inconsistent with one another. For example, a subject may start out classifying a given image as very intense, only to find toward the end of his observations that another image is noticeably more intense. In this case he naturally wishes to scale down all the other statements which he had made on the basis of his first relatively vivid image. The second procedure involves attempting to give the subject suggestions in which his attention is called to the possibility, for example, that the most vivid image might approach a perceptual experience in its character. It is also suggested that the faintest image would be almost impossible to detect. Suggestions of this sort are sure in the case of some subjects to be helpful, but others are not materially assisted and in a few cases the suggestions have proved confusing rather than otherwise. In the view of the writer these difficulties indicate simply that gradations of the kind mentioned are of very problem-

atic significance when gathered from untrained observers without the possibility of some measure of verification.

Gradations of stability would likewise be made on the basis of three distinctions; first, very stable, approaching perception in fixity; second, moderately stable; third, very fleeting. Here again by a system of plus and minus symbols the demand for a finer classification may be met if it arises.

It need not be argued that stability as a category of mental imagery is primarily applicable to the space mediating sensations. Images of sound are often in the nature of the case momentary and fleeting. In their case revivability would serve the same function in large part which stability serves in the case of the visual imagery. In a similar way olfactory images, when they can be gotten, are likely to be transitory, even though they may be recalled repeatedly.

Almost all subjects are able to report with confidence on the ease or difficulty with which they can command imagery. The gradations used may be 'very easy,' 'moderately easy,' 'difficult.' The plus and minus system affords adequate recognition of the differences met with in practice.

In the case of memory tests a grading of accuracy is instructive and the matter of detail may always be advantageously graded if gradation of any kind is to be attempted.

In the judgment of the author as was intimated a few lines above, the entire problem of gradation may well wait upon a more complete investigation of the qualitative peculiarities of imagery before attempting to define a definite line of procedure. At the present time it seems more than doubtful whether we can secure standards for this purpose so intelligible and so readily applicable as to enable us to secure uniformity among different investigators.

The author undertakes to offer no exhaustive bibliography of the subject. The references which have been cited in the body of the text are here brought together for convenience, and to them are added a number of other titles. The list contains the works which the author has found most directly useful, and which he believes present a just and essentially adequate record of the progress in the study of imagery since Galton's work.

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APPENDIX

After the foregoing report was in type, Professor L. J. Martin presented before the American Psychological Association, at its annual meeting held at Minneapolis during the last week of December 1910, an interesting account of some experiments on the projection of visual images.

Professor Martin put before her subjects common articles such as books, ink bottles and water glasses. They were then instructed to attempt to form and project a visual image of the object which was to appear just beside the perceptual original. The image and the percept were then compared. A considerable number of her subjects were reported as able to do this satisfactorily.

The tests in the present paper [Pt. II, Div. II E] dealing with the description of objects and pictures resemble Professor Martin's tests in some particulars and proved extremely illuminating, as has already been stated, when a comparison was made at the end of the test between the image and its perceptual original. Such comparison is obviously analogous, though by no means identical, with her procedure in requiring a direct comparison of the image with the object, the two being ostensibly present simultaneously. Her method certainly deserves further study. But it is clearly exposed to certain introspective inaccuracies which would need to be carefully safe-guarded, if the test were employed upon inexperienced observers. Our own results indicated far less capacity of projection in our subjects than in hers.

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Tests for Practical Mental Classification

By

William Healy, M.D.,
Director of the Chicago Juvenile Psychopathic Institute

and

Grace Maxwell Fernald, Ph.D.
Formerly Psychologist of the Institute

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APPRECIATIVELY DEDICATED

TO

MRS. W. F. DUMMER

WHO HAS MADE THIS WORK POSSIBLE

PREFACE

The work of the Juvenile Psychopathic Institute of Chicago is centered on the highly important problem of causative factors of the criminalistic career. For many reasons the study of the young repeated offender is to be regarded as containing much promise. In the first place, statistics have amply proven him to be the future criminal—lacking American figures on this point we may cite Matz, who states that among the entire prison population of the province of Pommern seventy per cent have received their first punishment during their minority and that of those repeatedly sentenced, by far the greatest number have received their first conviction by the time they were seventeen years old. The factors then which make for criminality are nearly always already present before the end of adolescence. In the second place, the young recidivist is definitely regarded as a problem by parents, teachers, and officers, who frequently welcome any light which may be thrown on the case. At this period in a criminalistic career it is easiest to get cooperation from parents and families in the study of the individual. Sometimes this is of the utmost importance, as when conditions of environment, heredity and more particularly of developmental history, which have great bearing on the case, are revealed. Thirdly, the individual during childhood or early youth is usually at a much better age for the ascertainment of mental qualities than at a later and less naive period. Fourthly, the study of the individual has a much greater chance of being valuable at this age, when remedial measures will find greater mental and moral flexibility, than at any later period in life.

The amount of light that may be thrown upon these personal problems, both as regards the welfare of the individual and the protection of society, can only be shown, of course, by case studies. Suffice it now to say that a glance at the

results of thorough-going studies will demonstrate their absolute necessity if the right thing is to be done at the right time for the heading off of a criminalistic career.

We of the Juvenile Psychopathic Institute have been most fortunate in our psychologist friends. At the inception of our effort the policies of our psychological work were shaped largely in accord with the advice of men who were in the best position to know what should be done, and during the progress of the last two years in the building up of our set of tests, we have had much occasion to feel grateful for help received in many ways in the shape of suggestions and encouragements and criticisms. It is hopeless to attempt to specify proportions and points of help received. Messrs. Angell, O'Shea, Dearborn, Whipple, B. T. Baldwin, Sidis, Sanford, Thorndike, Goddard, Sharp, Woodworth, Muensterberg, MacMillan, Freeman, Jastrow, W. E. Fernald, Bruner, Huey and J. W. Hayes have all been constructively helpful. To them the director of this Institute wishes to express his grateful acknowledgement. To Professor Angell we are deeply indebted for assistance of various kinds, which has been given ever since the earliest working plans of the Institute were under consideration.

After the Institute had been established for a few months Dr. Grace M. Fernald came in as psychologist and was with us for a year. To her is due much praise for patient effort in our gradual development of the tests as well as for the special points which are credited to her in the text. Miss Clara Schmitt at one time voluntarily gave much help. The present psychologist of the Institute is Dr. Mary H. S. Hayes. Her work has been most helpful towards perfecting methods of recording and scoring of tests.

The director of the Institute assumes entire responsibility for the text of this monograph.

January, 1911.

W. H.

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TESTS FOR USE IN PRACTICAL MENTAL CLASSIFICATION

INTRODUCTION

Diagnosis of mental capabilities and adaptabilities as far as this might be practically possible, was seen at the outset of the work of the Juvenile Psychopathic Institute to be one of the main desiderata in our study of individuals who are young members of the criminalistic class or who are otherwise repeatedly delinquent.

It appears perfectly plain to any one who thoughtfully views the general criminalistic situation and especially the procedure of courts which deal with offenders who are presumably more or less in the formative period, that the agencies intended to produce the desired reform are set in operation without any careful ascertainment of the actual needs of the individual as such. In other words, treatment is definitely undertaken without diagnosis. The most cursory inspection shows that many cases appear in our courts presenting extremely difficult problems. This fact, together with observation of the failures of institutional treatment, due frequently, as many institutional men say, to improper classification and disposal of the offender, is leading many people who are acquainted with first hand facts to the conclusion that, perhaps, one of the greatest causes of the lack of success in our handling of criminals has been our neglect of the study of their actual mental conditions, needs and adaptabilities. Particularly does it seem that both they and society could profit, if carefully adapted regenerative or protective measures might be undertaken at the age when habits and character are being set. As for ourselves, it has been borne in upon us that the great call is for some practical methods of accurately determining what really may best be done for this or that individ-

ual offender in order that society may cease to suffer from his or her delinquencies. Note, not what ought to be done *to* them but what ought to be done *for* them. The recognition that the protection of society lies in the application of this formula and that the failure of the past in the matter is largely due to unscientifically applied retributions and repressions is the key note of the new criminology.

There are, of course, a number of points of view from which this whole problem of the peculiarities and possibilities of the repeatedly delinquent individual may be approached—sociological, medical and psychological. We have outlined them elsewhere.¹ Even in the psychological field there are standpoints, especially of abnormal psychology, other than that which will be presented in this paper. However, we have been convinced that in order to do effective justice to the total situation involved in any given case, whether in court or not, there certainly must be in every instance an estimation for practical purposes and, of course, by practicable methods of (a) mental ability independent, as much as possible, of the results of formal education; (b) the extent and result of formal education; (c) the preponderating opportunities and interests, in the life of the individual [or lack of what might be rationally expected to be normal opportunities and interests], as shown by some study of mental content. It is with these three points that we are especially concerned in this paper. We must here, once for all, insistently disclaim that we think that under these heads one can find out, even in the majority of cases, all that is best to be done for the individual. We have seen many instances where the essential trouble was discovered only by inquiry instituted from an entirely different point of view. It is clear, at all events, that these three considerations, in the general survey of the individual and the factors in his development, must never be disregarded.

The desirability of psychological work in connection with courts which have to do with the disposition of criminals and

¹ Journal of Amer. Institute of Criminal Law and Criminology, Vol. 1, No. 1, May, 1910. Also 'A System of Recording Data,' Bulletin No. 2 of the same Institute.

delinquents, especially juvenile courts, is perhaps hardly necessary to emphasize to readers of this paper. But still it may be worth mentioning that the judge with his slender opportunities for observation and his lack of results of carefully conducted examinations is often in a poor position to do the best that could be done. We know too much about mental defectives who have the gift of language, appearing bright and with good enough presence to readily pass muster in court, we know too much of defectives who can do well under the prescribed conditions of institutional life, to believe that without competent psychological examination differentiation can be safely made between those whom it is wise for society to allow out on probation and those who in the interests of social protection should have proper institutional or other guardianship.

After nearly two years of continuous daily work in our Institute, which has been generously endowed and which has received a splendid amount of assistance from able psychologists, to whom grateful acknowledgement has already been made, we find ourselves using methods and a set of tests which we have for the most part ourselves developed. On account of their practical nature and the demand which exists for them, the time for publication seems ripe, *but it is to be distinctly understood that we ourselves still regard our tests and methods as strictly tentative.* We have tried out and discarded a good many tests which have been offered, or which we have devised, and it may be that it will prove desirable to eliminate some of the present series—we already know them to be of unequal value—or to add others. However that may be, our set has been developed as the outcome of careful, practical work. It embodies the results of repeated conferences with various psychologists who have been willing to give their attention to the needs of this bit of semi-public work in which we are engaged.

The wisdom of developing our tests along lines befitting our eminently practical purposes has been well proven. The early advice of a number of eminent American psychologists to avoid mechanical laboratory apparatus accorded with our

first survey of the necessities and ends of the work. We needed tests, not for the purpose of analyzing out or measuring the simplest mental elements or small differences, but rather for estimating the quality and quantity of the more general complex mental characteristics—particularly such as are involved in the intellectual and moral judgments, the performance of occupational tasks and other reactions of actual daily life; such mental characteristics, in a word, as might be suspected of being correlated with variations from the moral standards of society. One point has had to be carefully guarded all along—it is most necessary in such work with our cosmopolitan population to eliminate the language factor as much as possible. In predicting the possible development of an individual under various conditions it is most desirable to ascertain the mental ability quite apart from the individual's experience in formal training in our language, or indeed in any language. It often becomes necessary to classify mentally a subject who has had no education in English-speaking schools, or, indeed, who has had but little schooling of any kind. On one occasion we found ourselves able to demonstrate satisfactorily that a gypsy boy of 15, quite innocent of schooling and knowledge of the 'three R's', had at least fair, if not good, native ability. And repeatedly a number of our tests have proven most serviceable in mentally classifying young deaf and dumb children. Altogether we know that by the use of these tests and inquiries most efficient aid is obtained in the making of diagnoses which lead to recommendations for the handling of the individual, but, no doubt, we shall ourselves in the future find improvements. In the meantime we specifically invite suggestions and criticisms to the end that better work may be done all along the line on this vastly important subject.

Our initial investigation into this field showed no set of mental tests at all adequate to give the desired information about the capabilities of delinquents. We found that the average age of the repeated offender to be studied was 15 years, while we must occasionally see children as young as 8 years old, and that sometimes we should also see young adults.

These individuals range in mental capacity all the way from imbecility to those who seem considerably to excel the ordinary person of their age in ability and information. We have had to evolve practical methods for estimating this entire array of abilities except for those of children under 10 and for the definitely feeble-minded. In working with these two latter classes we have used the Binet classification, especially for the diagnosis of the institutional type of feeble-minded. Like other workers in this field, we have found this system extremely valuable, but still presenting a number of imperfections. It helps very little where the language factor is a barrier, either on account of foreign parentage or insufficient schooling, and with uneducated deaf and dumb children. Of the tests which form our own set, a few have been well worked up by other investigators before us. Proper acknowledgement will be made in each instance.

Our tendency has steadily been towards making our use of tests and inquiries more and more thorough. Experience teaches us that a cursory examination may lead to unsound conclusions which in turn may be made the basis of thoroughly unadapted attempts at betterment. At the same time, perforce of circumstances, we have been obliged, as every other worker will be obliged, to keep within practicable working conditions. This means that while one may be able to spend the equivalent of an entire day's work on one case, or occasionally the equivalent of two or three days' work, a longer time will probably be found almost everywhere impracticable. We are by no means sure that for the first diagnosis it will be found necessary. Almost any reasonable expenditure of effort would be economical, of course, to head off a criminalistic career, but secondary to the original estimation of the individual and his needs there should be, whenever possible, follow-up work in the shape of evaluation of changing phases of the individual's development and of the results of ameliorative measures, which may have been instituted. We have striven always to make tests, not for the test's sake, but for the sake of knowing the individual.

Since one of our tests has only recently been evolved, and

some have lately been modified, and in still others new methods of scoring have been devised, while, on the other hand, certain ones have been long used by us as well as by others before us, it follows that our knowledge of the norms for the different tests is exceedingly unequal. However, we know that no one of them is too difficult and that none of them involves too much time in getting positive results. We are able to give the working limitations for even the newest of them, but the development of actual norms is a matter for the future and will be a separate contribution. The critical findings of other investigators will, we hope, be of great value. As it stands now, what the set of tests and inquiries offers is an opportunity for making a practical, work-a-day classification and estimate of capabilities that shall tend to throw valuable light on what ought to be done with those individuals who are distinct problems from a normal standpoint, and this whether the case is to be adjudicated by the officers of the law or handled by family guardians.

Concerning the possibility of getting hold of the individual in the right way for ascertaining the scientific truths desirable, we ought to say in explanation and for the encouragement of those who contemplate anything like a similar endeavor, that we have been overjoyed to find the heartiest cooperation both on the part of parents and of children. There were some doubts at the outset about possible disturbing emotional attitudes and interfering recalcitrancies, but we have found these untoward possibilities practically negligible.

We have discovered at least two phenomena that have worked greatly in our favor; the first is that the individual before his case is adjudicated in any way, either in court or out of it, is, in the vast majority of instances, peculiarly keen to show the full extent of his ability. On several occasions we had the experience of finding that the individual already committed, we will say to a reformatory, was a totally different being from one whose case was pending, or who was on probation and who had the possibilities and hopes and encouragements of the future before him. Then, second, we

also found that there seems to be a certain age, varying considerably in different individuals, but below which one can expect naïveté and a peculiarly frank attitude toward the world, even in some of the morally worst of the boys and girls, and beyond which age, as a rule, except by dint of much greater effort, one can not hope to develop the friendly relationships which are necessary for getting the scientific data. Below this age, whatever it may be, is the time of responsiveness, openness and usual truthfulness—prevarications then being often of such ingenuous character that they are readily recognizable—beyond that period is secretiveness, aloofness, the development of a definite attitude, it may be of grudge, toward the world; the individual has often built up a wall in front of himself that is hard to break down. A little experience on these points would demonstrate to many a criminologist where his work ought to begin.

It seems that nearly always the approach to the tests is much less self-conscious in young people than in adults, and this not altogether on account of the fact that they are constantly being asked for response to tests in school, for we have observed it in children who have been to school little or not at all. With the idea of invoking always as much interest as possible in our tests, we have ever had in mind the development of them in forms resembling games and puzzles, but really involving points much more open than puzzles to solution by use of simple reasoning ability. This has seemed highly successful, for it has been our frequent experience to have our subject want to do the things all over again. Another condition for achieving success must never be lost sight of, that is the presentation of a winning and encouraging attitude on the part of the investigator. Binet is right; if one wishes to get tests fairly used there must be constant encouragement and stimulus, even in the face of poor results, provided earnest effort has been made.

It is of considerable importance in most instances to have the school record of an individual in order to evaluate his present standing in regard to both information and the results of formal education. Naturally, the type of school an indi-

vidual has attended, whether or not his learning has been mostly in a foreign language, as is the case with some of our subjects born in this country, whether there has been much truancy or absence from other cause, the type of instruction in that particular school, the attitude of the teacher and the individual towards each other, the age at which schooling was begun and ended, the grade reached and whether that grade was the standard of the good school system, if retardation in school, what cause was assigned—all these are factors about which much can be frequently ascertained through the family, or the officer in charge, or directly from the teachers and principals.

The order in which the following tests are given may be varied according to the needs of the given situation, and particularly to the apparent interests evoked during the examination. We have arranged the set with the idea of grouping for expository purposes those tests which involve the same general forms of mental activity. To avoid fatigue and keep up interest we specifically advise against giving them in this logical order. Very frequently we find it necessary and advisable to divide them for different sittings and such division is made quite irrespective of any given grouping. To hold attention and interest seems quite the most essential condition. With these precautions we are seldom bothered by apparent fatigue, but, of course, one should be on the lookout for it. The only test which is intended to be given in a definite order is the one so specifically named, the introductory puzzle.

In common with other workers on mental tests, we insist on the necessity of our tests being made exactly as we prescribe and being used precisely as we stipulate, if anything like comparative results are aimed at. We have seen one of our designs, for instance, made by a competent laboratory mechanic, which would not conform to the apparently small requirement that certain parts be interchangeable. In consequence it failed entirely to bring out the very point that we found most valuable in estimating the ability of the subject. The entire set is quite inexpensive and is readily made

by any one with even a moderate amount of mechanical ingenuity. The description of each, it is hoped, will be, accurate and ample enough to enable any qualified investigator to make, or to have made, the test apparatus so precisely that every point of virtue may be thoroughly brought out.

Methods of scoring will be, of course, exceedingly important in connection with the use of each test. One of the valuable bits of work done in the Institute has been in estimating the kind of successes and kind of failures that have been made in the various tests. No single method of scoring could be regarded as generally valid and so in each case the treatment of the results obtained has had to be gradually worked out. The methods of scoring will be carefully specified in connection with a description of each test. The meaning of these separate points we hope will be gradually made clearer by further work. It may be that for some tests a system of percentage marks, calculated upon a basis of differently weighted details, will be found most desirable. But as a matter of fact, we think it will be clear to any one who applies the series, that even the rougher results obtainable from the use of the set as a whole, as it now stands with the method of scorings given, are most valuable from a practical standpoint. Reference to the chapter on classification will give some indication of practical findings.

For the purpose of showing the practicability of such a set of tests as ours, it seems desirable to give a concrete illustration of the working up of a case from this standpoint with the findings under each test. A subject who may be designated as Case 574 will be taken. This is a city boy, 15 years old and of American parentage. This particular individual has been selected not because he represents, except as to age, any sort of an average—it is as difficult a matter here as elsewhere to speak of an average individual—but simply because he represents in difficulty just such a problem as is handled every day by Juvenile Court officers. Physically he is rather under-developed and nourished, has a poorly developed chest and poor color. He has a frequently recurring habit spasm of the muscles of the eye lids. No trouble

with the special senses noted. Expression rather pleasant, manner polite, but he looks much worried and cries at times bitterly. The main points in his developmental history are that he had scarlet fever six years ago and was then very ill. Before that time he had an attack of chorea which lasted long and four or five years ago he had another prolonged attack. The twitching of his eyes began a couple of years ago. At ten years of age he began smoking; when with his street companions he frequently smoked from ten to fifteen cigarettes a day. The father died six years ago. He was a good man. The mother is a fairly intelligent and hard working woman who has moved away from the home which her husband left her, and where they had lived for fifteen years, because this boy was going with bad companions in the neighborhood. Last summer she put him with his brother out in the country for a couple of months and when there, the boy did well and did not smoke. No indication in any other way of other bad habits except occasionally playing with dice. The boy passed for eighth grade in the public school. He had left school some ten months prior to the time we saw him. The longest he has worked in any one place since then has been one week. In the meantime he has been going pretty steadily with the companions with whom he got into trouble before he left school. With them he has been engaged in some petty thievery. The mother and the officers have regarded this boy as a pretty serious problem, particularly because he seemed to be very smart and yet in spite of repeated promises to the judge and others, repeatedly got into trouble that would seem readily avoidable. As the mother put it, "What could be the matter with him that he should show so little sense. Had his nervous trouble affected his mind?"

The reader who looks over the results of the tests on this case should realize that the records are nearly all exceptionally good even for our fifteen year old subjects. From a psychological standpoint we decided that this boy was in ability considerably above the ordinary individual we see. Proportionately, his information was extremely narrow in range, particularly lacking was the interest in the things which healthy minded boys are usually most fond of.

DESCRIPTION OF TESTS

TEST I. INTRODUCTORY PICTURE FORM BOARD

The novel method of combining the idea of the test form board with the picture puzzle was developed because of the obvious interest there might be in such a test and because in the doing of it various elements of mental life are brought into play. This particular pattern has worked out successfully as an introductory test. It enables the investigator often to get a rough estimate of the subject's whereabouts in the scale of mental ability and it furnishes the latter with a task in which he is interested and in which, unless he falls low in the grade of the feeble-minded, he always has, at least, some measure of success. It is readily seen that the test brings out what the ordinary form board brings out, viz., perception of differences in form, powers of coordination in handling the pieces, the ability to learn by the experiences of trial and success and that beyond this, it may afford some gauge of the perception of the relationship of object to object, of parts to the whole—a most valuable faculty in life. If a boy observes, 'Oh, gee! that dog's caught a mouse', or 'There's a baby horse standing by its mother', one gets some impression of the subject's mentality. But if the attempt to put an animal's head in upside down is persistently made, that likewise bespeaks certain mental characteristics.

The design shows a certain number of pieces cut out on the natural lines of some of the objects in a picture together with four other pieces, one of which is irregular in shape, and three of which are cut on geometrical lines. Two of these last somewhat resemble each other, but are not interchangeable. The other geometrical piece is an equilateral triangle divided into two right angle triangles. This last was particularly to provide for a simple trial and error procedure, if the make-

up of the parent triangle was not at once recognized—as it usually is not. The plan is readily perceived from Figs. 1 and 3. This design as well as a number of our others, as will be apparent from the illustrations, may be made very readily and serviceably from selected, 3-ply, scroll-saw wood. For this introductory test a suitable picture, about 8 by 11 inches, from a child's picture book is carefully glued with strong glue to the wood. The pieces are then readily cut out with a scroll or bracket saw.

The method of procedure is worth noting here, not because this test is regarded in any way as one of the more exact of our series, but because it is representative of the general precautionary methods, which must in fairness be followed in all of them. To begin with, the subject is told in a general way what you are going to do with him in the examination. We often tell him we wish to see how good he is at doing things, how exact and how quick, and we often first show the stop-watch, nearly always an object of much interest, at least to boys. Then the picture with its empty spaces is put before him in good light. At its side, spread out right side up and well mixed, are placed the separate pieces. He is then told that here is an easy picture to put together and you want to see how quickly he can do it. Most of our twelve year old children succeed in doing this test in from one to three minutes.

Scoring: The variety and possible combination of mental elements which may be used in putting this picture form board together is so great that standardization for scoring, except in very rough ways, is undesirable. Time is of course taken and is registered from the moment the subject gets to work, but much more important than the time is the study of the method by which the task is done. Occasionally the degree of facility of muscular coordination is worth noting. By far the commonest difficulty is in correct placing of the two right angle triangles in the parent equilateral triangle. The most frequent error is that shown in Fig. 2. The enumeration of the points for scoring, with their respectively designated numbers is as follows: (See above.)

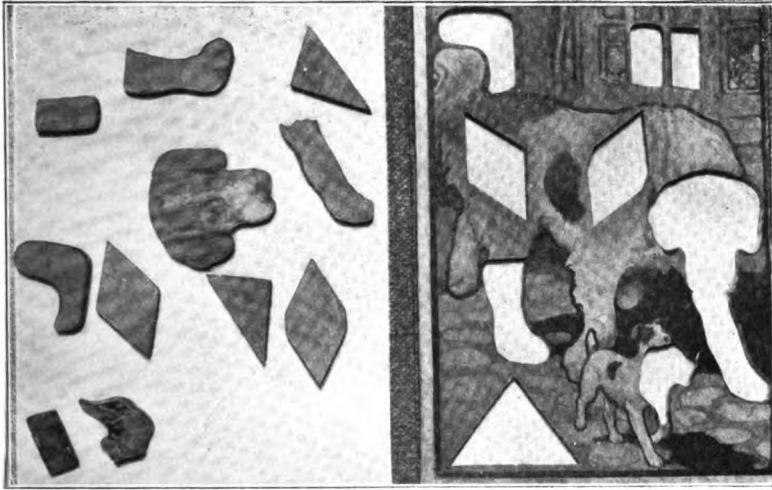


FIG. 1

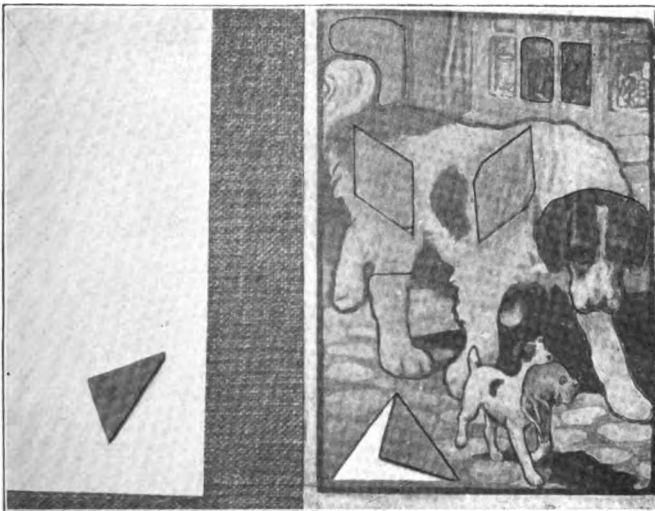


FIG. 2

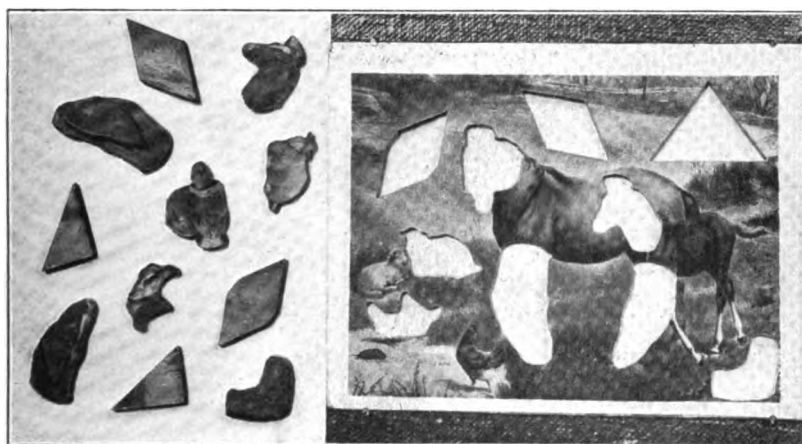


FIG. 3

1. Time—(if the problem is not solved at the end of ten minutes, it is considered a failure and recorded 'F', or in cases where the child gives up and can not be induced to continue, the letter 'F' is recorded with the time, for example, F-6' 20'').

2. Poor perception of form; for example, trying to place a triangle in a curved opening.

3. Persistent effort to force parts into impossible openings.

4. Noticeable trial and error in working with the double triangles.

5. Noticeable repetition of impossibilities in working with the double triangles. (This test is introductorily given to invoke interest and to gain general impressions, so it does not seem necessary to quantify the term 'noticeable'.)

The record on Case 574 is:

1. 2' 5''
2. No.
3. No.
4. No.
5. No.

TEST II. SPECIAL PICTURE PUZZLE

This test was designed to show primarily the apperception of the relationships of well defined and easily recognized parts to a given whole. Beyond this it, of course, roughly demonstrates sensory discriminations of form and color. The import of this test can not be brought out except by its careful construction. The pieces are properly sawed out with a certain amount of bilateral symmetry in pairs, so that one can not readily put them in position when guided by form alone. But on every piece there has been preserved a portion of the surrounding picture, which by lines or color or both, will readily give the clue to where it belongs.

This test, again, can easily be made out of three-ply scroll-saw wood. The picture is that of a school room with eight scholars and a teacher, and is taken from that entertaining child's book, 'Jingleman Jack, A Book of Occupations,' by

O'Dea and Kennedy.¹ It should be noted in this, as in Test I, that the pieces are not cut out, as in the ordinary picture puzzle, on arbitrary lines, but represent actual parts, such as a head or an arm, in this case with a little piece of the surrounding picture. All this is clearly indicated in Fig. 4.

The test is presented to the subject with the parts well mixed up, as in the illustration, and the individual is told that here is the picture of a school, with heads and arms and things cut out and you want to see how quickly and correctly he can put them together. He is told to pick up one piece at a time, look at it carefully, and then put it in place, and that if a wrong place is tried, it will be counted an error.

Our normal 12 year old children generally do this test in from one to two minutes and make from none to eight errors. The number of errors usually made by an individual poor in general ability is quite astonishing in the light of the apparent easiness of the task. Time is here again comparatively an unimportant factor. Of undoubted great significance are the errors scored under heading three.

- Scoring.*
1. Time.
 2. Number of errors.
 3. Number of errors of impossible situations;
for example, effort to put piece in upside
down.

The record of case 574 is:

1. 1' 50''
2. 5
3. 0

TEST III. CONSTRUCTION PUZZLE (A).

The first sketch of this test was given us by Professor F. N. Freeman. We, however, added the idea of making the pieces partially interchangeable. The significance of the results obtained largely hinges upon this interchangeableness.

¹Published by Saalfeld Publishing Co. Akron, Ohio, 1901.

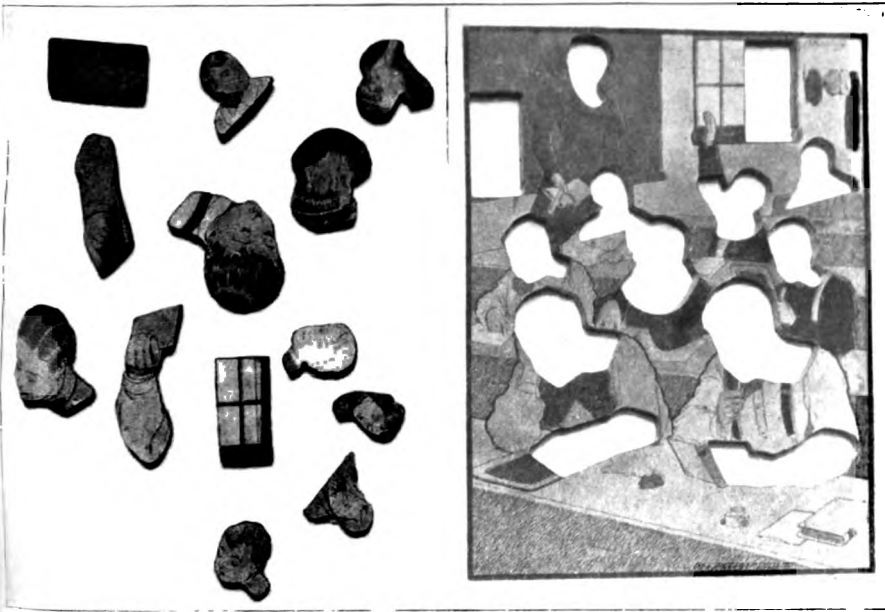
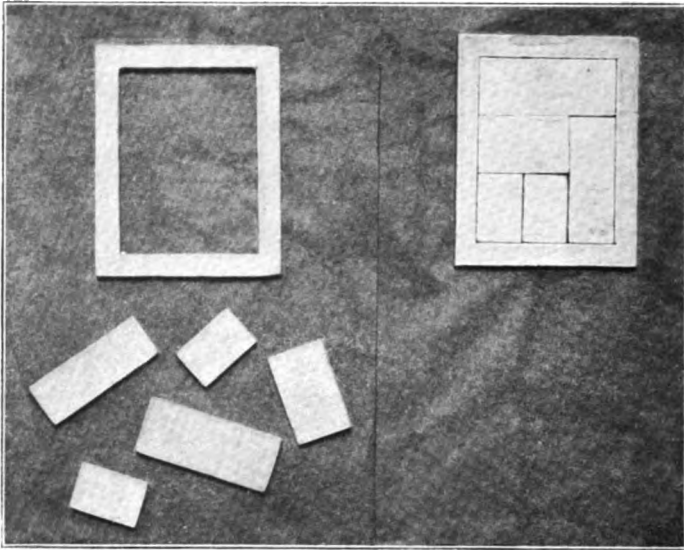


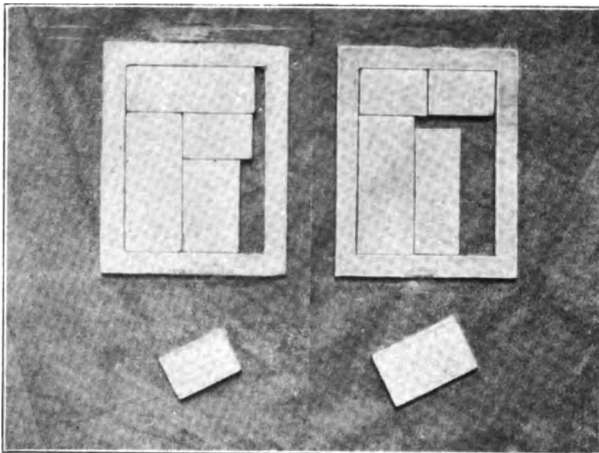
FIG. 4



a

FIG. 5

b



a

FIG. 6

b

This test brings out perception of relationships of form and also the individual's method of mental procedure for the given task—particularly his ability to profit by the experience of repeated trials, in contradistinction to the peculiar repetition of impossibilities characteristic of the subnormal and feeble-minded groups.

The test is readily and serviceably made of scroll-saw wood. The inside measurements of the empty rectangle are 4 x 3 inches. The subject is offered the test with the separate pieces irregularly disposed, as shown in Fig. 5 a, and is told that the space can be exactly filled up if they are put in correctly.

It would seem quite apparent that for estimating mental ability the method pursued in this task is of much greater value than the actual time. Probably all would acknowledge that a trial method, where the subject proceeds intelligently from one apparent possibility to another, even though a relatively long time is consumed, will not necessarily indicate lack of native ability. We find that most of our twelve year old children do this puzzle in time ranging from 12 seconds to 2 minutes.

Scoring is simplified by the following procedure: Each piece put in the frame and left there is easily recognizable as belonging under one or more of the heads described below under 2, 3, and 4 and a check mark may be placed in an appropriate column.

1. Time. (Failure recorded as in Test I.)
2. Number of moves made. (Five being the least number in which the task can be accomplished.)
3. Number of moves of obvious impossibilities, i. e., cases in which a piece is left in an evidently impossible situation, that is, where it leaves a space obviously unfitted to any of the remaining pieces,—for example, the narrower spaces shown in Figs. 6 a and 6 b.
4. Repetition of such obvious impossibilities.

Record of Case 574 is;

1. 19"
2. 7
3. 0
4. 0

TEST IV. CONSTRUCTION PUZZLE (B)

This test is the design of Dr. Grace M. Fernald, following the suggestion of Dr. W. F. Dearborn. Its purpose is to show the individual's perception of relationships of form and also to bring out his power to plan a bit of work, that is to say, his ability to see the possibility or impossibility of situations before they are actually attempted. The ability to profit by the experiences of trial and success or failure is so important, that for its estimation it seemed distinctly worth while adding a somewhat harder task of the type of Test III. When the performance has been remarkably good in Test III. we have occasionally felt sure that it was partly due to chance and so wished to carry the investigation further on this point.

The pattern is easily cut out of scroll-saw wood, but it is absolutely necessary that the parts be exactly made so that they are interchangeable throughout. Otherwise the significance of the results will be lost. The standard width of all the spaces in the pattern is one and three-eighths inches. The spaces which have only one rounded end are five inches long and the rectangle is two inches long. From these dimensions and the pattern as shown in Fig. 7 the design can be readily drawn.

The test is presented to the subject with the pieces well mixed up as in the illustration. He is told that if the pieces are put in correctly they will exactly fill all the spaces and he is to see how quickly he can put them in their proper places. In this test, again, it seems to us that a trial and success method can not be regarded as at all derogatory to native ability, but it does seem clear that in such a procedure the constantly getting of one's self back into old impossible situations is, on the contrary, evidence of poor ability. The shrewdest method pursued is to eliminate the small pieces which can only fill up certain definite spaces. Some of our subjects deliberately do this. A common result of faulty placing of the pieces is shown in Fig. 8. Time, again, in this test is hardly to be considered so important as estimation of the method pursued. Most of our twelve year old children are successful in from one to three minutes.

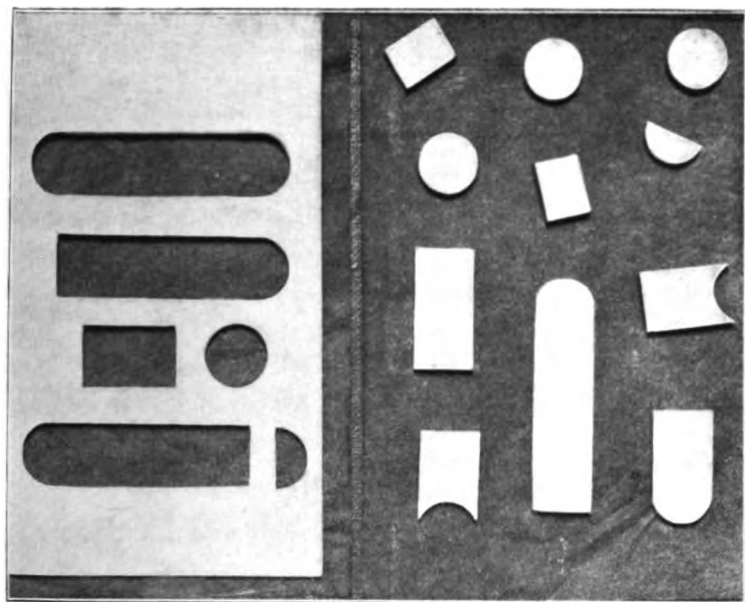


FIG. 7

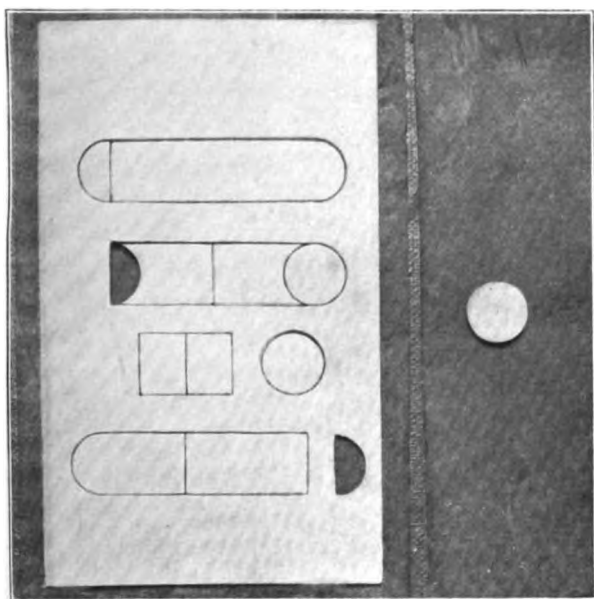


FIG. 8

Scoring; A very ingenious method of recording the actual number of moves and their significance has been devised by Dr. Mary H. S. Hayes. For this purpose the separate pieces are designated by numbers, which, since they come in order downwards, are easily remembered. With a rough tracing of the empty spaces an accurate and self-explanatory score sheet may be very easily filled in. The small exponent by the side of the piece-number shows the serial order of the move made. A line drawn through a piece-number means that it was removed and the exponent at the end of the line shows the number of the move. The method is shown in Fig. 9.

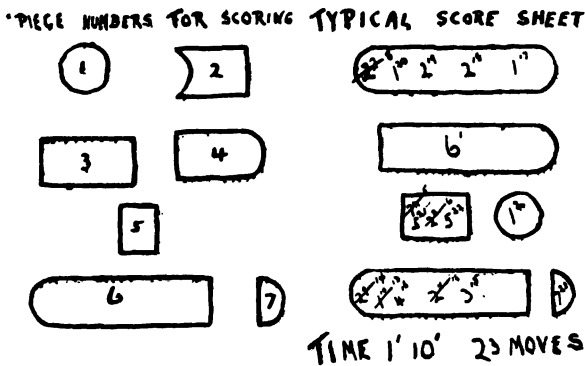


FIG. 9

The points for final record have not yet been determined by us because this improved method of graphic scoring we have only recently adopted. Prior to that we registered time and made a rough estimation of the intelligence of the procedure. In a later contribution on the standardization of the tests we hope this will be covered. For typical, graphic record see Fig. 9.

Record of Case 574 is;

Time; 55" Moves; 19.

TEST V. PUZZLE BOX

The development of this test resulted from suggestions received from a number of sources—the animal experimenters, and Thorndike, Woodworth and others. The box at present in use is one somewhat modified from the general design of Dr. Grace M. Fernald, whose original plan was of a box fastened by a hasp, staple and bolt which could be opened only by following a definite sequence of five or six steps. These steps consisted of the manipulation or removal of fastenings both outside and inside the box, the latter being accomplished by means of some tool which could be thrust for that purpose through certain openings—the whole procedure to be learned and studied by direct observation. The inside could be seen through a glass cover. Later modifications have not affected the general idea, but have changed the procedure and made the box much stronger and more stable as a working test. Mr. Joseph W. Hayes has most kindly contributed these improvements.

The purpose of this test is obvious. It may bring out abilities or defects in manipulative powers, in the ability to analyze a slightly complicated physical situation, in powers of attention and continuity of effort.

The description of this box is best made with reference to the illustration, Fig. 10, and to letters which may serve to designate the sequence of steps in the opening of the box. A curved bolt-hook (*A*) on the front side of the box is held in place by a ring (*B*) to which is attached a blue string running through a hole in the side of the box and up through another ring (*E*) and ending in a ring (*C*) which goes over a perpendicular arm of a post (*D*). Directly opposite to this is a hole in the front side of the box for manipulation by the long button hook, which is used as a tool. This blue string is held taut by the ring (*E*) which is in turn attached by an orange colored string to a ring (*F*), which is seen fitting over a post (*G*). A cross piece prevents this ring from slipping down too far. Opposite to this post there is a hole in the back side of the box. The last mentioned ring (*F*) is held in

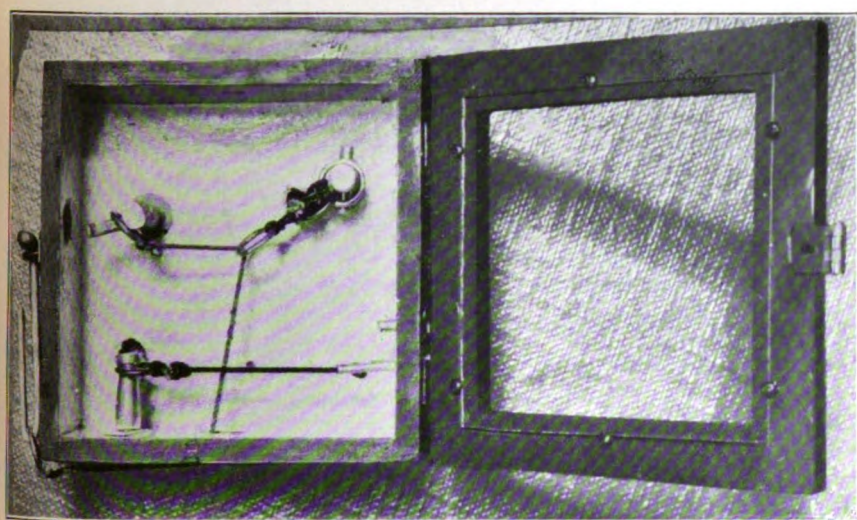


FIG. 10

place by a green string, which passes through a hole in the floor of the box, which may be seen near the post (*G*). This green string now passes under the box and ends in a ring (*H*)—not to be seen in the illustration—which is held in place by having the short arm of the staple (*I*), when released from the box, passed through it. The staple (*I*) is held tight by a red string attached to one arm which ends in the ring (*J*). This ring slips over a post (*K*) which is fastened to the side of the box. The hole for the manipulation of this last ring is in the floor of the box directly below the head of the last mentioned post.

The color of the strings is, of course, arbitrary and is made different in order to facilitate the tracing of the sequence of events necessary in opening the box. One removes first the ring over the post (*K*) and pulls out the staple from its holes in the back of the box, releasing the attached ring. Next the ring over the post (*G*) is lifted off, which loosens the short orange colored string so that the ring on the arm of post (*D*) can be readily removed. This then so loosens the blue string that the final ring can be pushed over the curved arm of the bolt-hook and the latter may be withdrawn, the hasp lifted and the box opened.

The box itself must be strongly made. The bolt-hook, posts and staple may be made of aluminum and the posts are securely held in place. The procedure in giving this test is comparatively simple. The subject is told that he should look the box over thoroughly before beginning and that he can use the button hook. No parts must be cut or destroyed. He should be told all this and also that if he does the right things in the right order the box may easily be opened. Finally before putting it in his hands, one should say, "Now see how quickly you can get it open."

It is obvious that the general results obtained from this test must vary greatly, but there seem to be three main types of approach to the problem; first, random trials; second, intelligent profiting by the experiences of trials and successes or failures; third, conscious analysis of the puzzle as a whole with recognition of the relation of the parts. Of course, on

account of the differences in strength and manipulative power there would, other things being equal, be considerable difference in the times taken by the subjects. Indeed, altogether it has seemed to us that the method employed by the subject is of more significance than the time. Most of our twelve year old subjects have opened the box in from one and a half to nine minutes, but a certain number have finally failed.

Scoring; 1. Time. (If the problem is not solved at the end of 15 minutes, it is considered a failure and recorded 'F', or in cases where the child gives up and can not be induced to continue before that time, the letter 'F' is recorded with the time.)

2. Attitude of subject.

- (a) Interested and making an effort to solve the problem.
- (b) Helpless turning around of the box and protesting he can't do it.

3. Method of procedure.

- (a) Point of attack—
- (b) Step 1. Removing ring J from post K—time spent in doing it.
- (c) Step 2. Pulling out staple I and releasing ring H—time spent in doing it.
- (d) Step 3. Removing ring F from post G—time spent in doing it.
- (e) Step 4. Removing ring C from arm of post D—time spent in doing it.
- (f) Step 5. Removing ring B from hook A—time spent in doing it. *
- (g) Step 6. Removing hook A and opening box—time spent in doing it.
- (h) Error 1. Tugging at bolt-hook A out of order—time spent in doing it.
- (i) Error 2. Trying to remove ring B out of order. Time spent in doing it.
- (j) Error 3. Trying to remove ring C out of order. Time spent in doing it.

- (k) Error 4. Trying to remove ring F out of order.
Time spent in doing it.
- (l) Error 5. Trying to pull out staple I out of order. Time spent in doing it.
- Additional errors, such as tugging at the strings, are noted in the record.

Record of Case 574 is;

1. 1' 25"
2. (a) Yes.
(b) —
3. Point of attack Ring J
 - Step 1. 10"
 - Step 2. 5"
 - Step 3. 15"
 - Step 4. 2"
 - Step 5. 5"
 - Step 6. 5"

Record of Case 573, (girl, 17 years old,) is;

1. 4' 6"
2. (a) Poor interest.
(b) —
3. Point of attack Ring F
 - Error 3. 15"
 - Error 2. 10"
 - Error 3. 5"
 - Step 1. 15"
 - Step 2. 10"
 - Error 2. 40"
 - Step 3. 20"
 - Step 4. 20"
 - Step 5. 15"
 - Step 6. 5"

TEST VI. 'AUSSAGE'—TESTIMONY FROM A PICTURE

This is our adaptation of one of the tests of those German psychologists, particularly Wilhelm Stern, who have done so

much to develop a most important line of research on the individual's ability to give accurate testimony.

The purpose of this test in our hands is to discover the power of the subject to report faithfully what he has seen. We find that incidentally we may sometimes ascertain also certain facts that throw light not only upon sense perceptions and recall, but upon other mental characteristics which might possibly have to do with the moral make-up of the individual. These are suggestibility, imaginativeness, powers of dramatization, unswerving honesty of report and so on.

The only picture thoroughly adapted to our needs that we could find after long search was of a butcher shop, Fig. 11. This likewise was taken from "Jingleman Jack"*. Every child whom we have seen has been familiar with such a shop and most of the objects depicted in the picture.

The method we pursue is to tell the subject that we have here a picture of a butcher shop, and we are going to show it to him for a short time. In that time he must study it thoroughly. After that he must tell us all he has seen and then we will ask him all sorts of questions about it, about things that were perhaps there and perhaps were not there. Then we place the picture in his hands so that a good light falls upon it. He is given from ten to fifteen seconds to look at it, and then, quickly taking it away, we ask him for his own account of what he saw. This is written down almost verbatim. After he is through with his free recital, we carry on a kind of cross examination, which is calculated to bring out details of the picture which he may have forgotten, as well as his suggestibility. During the course of this cross examination, we definitely ask about unmentioned details of objects and actions, about the colors of the butcher's shirt and hat, of the dog and of the woman's dress. To bring out his possible suggestibility we get him to name all that he saw hanging on the side wall and then, after asking another question or two, inquire if he saw the bunch of bananas. Bananas are not in the picture, neither are the elec-

*Vide Test II.



FIG. 11

tric light, nor the box for scraps, nor meat visible in the ice-box, nor saw-dust on the floor, all of which we inquire for while we are asking for the other things which are really there. The answers are all written down as memoranda by the investigator.

Often this test brings out very interesting results with regard to the power of recall and the adventitious use of the imagination, with regard to suggestibility, veracity, and, rarely, power of dramatization. Occasionally it has seemed that the results obtained have had some correlation with the delinquency, as in the case of extreme mendaciousness. Subjects are nearly always interested in this test. The results obtained differ extremely, but sometimes we seem to have very definite types of response. There is the down-right honest response from the subject who is sure that he recalls nothing but what was there and who if he is not certain that he has seen a thing will say so. There is the shrewd response given by a subject who seems to calculate on the probability of some of the suggested objects, saying, as it were to himself, 'I'll tell them it's there because they ask for it and it may really be there.' Then there is the loquacious lying response offered by the subnormal individual who apperceives very little indeed of what there is in the picture. As a contrast, we find the subject who visualizes the whole panorama of events and with word and gesture vividly dramatizes the situation. We know that we are justified in trying to estimate some phases of suggestibility in this test, because of the many individuals even of twelve years old or less who are quite sure that the objects suggested to them are not present in the picture. The significance of the accepted suggestion or of the testimony in general may be very complex, but when one notes the sturdy responses often obtained from children who are both bright and honest, one must believe that the differences between these results and the markedly inferior reports may mean a good deal.

Scoring; From the memoranda jotted down while the examination is in progress, the following points seem worth scoring.

1. Number of details.
 - (a) obtained by free recital.
 - (b) imagined details.
 - (c) correct details obtained by questioning.
 - (d) erroneous details.
 - (e) details of color, { correct.
incorrect.
2. Mode of response.
 - (a) Enumerative.
 - (b) Functional. In cases where the response is partly of one kind and partly of another, it is recorded by means of a fraction—for example, (a) $\frac{1}{2}$, (b) $\frac{1}{2}$.
3. Suggestibility.
 - (a) Number of probable suggestions accepted.
 - (b) Number of improbable suggestions accepted.

Record of Case 560 is;

"Saw butcher with sausages in hand and some lying on counter to be wrapped up and chopper with cleaver. Meat hanging; legs of beef. Woman had basket, girl had something—bread. Dog was reaching up sniffing at sausage. Other meat besides. Don't know any more."

Cross exam.—Butcher is spry-looking man, had smile. Not large man, thin looking, apron on, strap went over his shoulders and around him, had lots of hair, no cap. Sausages in one hand, knife in the other. Scales weren't in there. Ice box—no. Don't know meat on side wall. Think she had pocket book in other hand. Only one knife seen.

Color—shirt—not noticed. Dress—sort of brown. Dog—white, had spots on him. Butcher's hair—brown.

Suggestibility—o.

Non-suggestibility. Bunch of bananas. Box for scraps. Meat in ice box. (Didn't see ice box.) No electric light. Didn't see sawdust.

1. (a) 14
 (b) 2
 (c) 7
 (d) 5
 (e) correct 1
 incorrect 2.
2. (a) $\frac{1}{2}$
 (b) $\frac{1}{3}$
3. (a) 0
 (b) 0

TEST VII. VISUAL MEMORY OF GEOMETRIC FIGURES

This is a well known Binet test and forms one of the simplest of our series. It is usually denominated a visual memory test, but particularly in the second figure the observer can often easily determine that the motor method of learning and recall is partly used.

The designs as shown in Fig. 12 are drawn in heavy black lines and the dimensions of the large rectangle are about

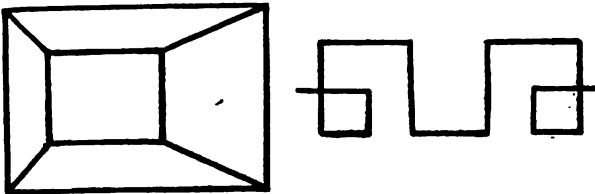


FIG. 12

3 x 2 inches. The total length of the other design is 3 inches, width 1 inch.

The subject is told that we are going to show him a figure for a little while and we want him to draw something that looks like it. We tell him he need not take much time to draw it because we do not care if it is not drawn well. We simply want him to show us what it looks like. The rectangular design is shown for five seconds, is withdrawn and the child told to draw it. If there is any question, as there sometimes is, as to whether he means the inner figure to be

represented as in the center of the large figure or not, he is asked to tell its relative position. If the figures are extremely poorly drawn, he may be asked to repeat after another look at the model. The second figure is exposed for five seconds in the same way.

The commonest error in reproducing the first figure is due to the lack of perception that the second rectangle is not in the middle of the first. The commonest error in the second figure is due to the lack of symmetrical reproduction of the parts. In the attempt to learn this last figure it may be often observed that the subject's eyes follow the lines and his fingers trace them in the air. The first figure is reproduced correctly by most of our twelve year olds, but a less number are successful with the second.

Scoring; the result of the test itself is its own record. Scoring points are as follows for the two designs (a) and (b):

- (a) 1. Correct, viz., result is recognizable as intended for a representation of the model.
- 2. Correct except for error in placing small figure in center.
- 3. Failure, viz., result not recognizable as representation of the model.
- (b) 1. Correct, viz., result is recognizable as intended for a representation of the model.
- 2. Correct except for lack of symmetry.
- 3. Failure, viz., result not recognizable as representation of the model.

The record of Case 574 is:

- (a) 1. Yes.
- 2. —
- 3. —
- (b) 1. Yes.
- 2. —
- 3. —

TEST VIII. LEARNING TEST—ARBITRARY ASSOCIATIONS

Our method in this test is a variation on suggestions offered by Whipple, Dearborn, MacMillan and Bruner, all of whom have had experience with somewhat similar tests.

The purpose in this is to get a gauge of the powers of attention and the ability of the subject to establish a comparatively easy set of associations.

1 2 + ³ Δ = ⁵ L X O C

O) Δ

+ = X

L) C

X □ =

) L Δ

+ □ C

Δ X)

O = C

L + □

+ O Δ
) □ X C
 =) L

FIG. 13

Nine clearly made symbols with numbers attached are presented together with 27 similar, but unnumbered symbols, three of each kind. These blank symbols are given for the

subject to fill in as part of the learning process. On a portion of the sheet, which is turned under until it is used, there are placed ten unnumbered symbols, one being repeated in order to gauge the intelligence of reaction to that particular phenomenon. The make-up of the whole pattern can be plainly seen in Fig. 13. The subject is asked to fill in the numbers of the 27 practice symbols and after he has done this, he is told to study well the top line until he thinks he knows it. Then the sheet is turned over and the subject is told to fill in the numbers from memory.

We find plenty of children even of eight and ten years quite able to do this task without errors. Experience with this sort of test indicates that it is done relatively more readily by the child type of mind than by the adult type.

- Scoring:*
1. Number of errors or omissions in graphic learning.
 2. Number of errors or omissions in reproducing.
 3. Placing the same number in different figures in the reproduction, for example, the number 4 in both the triangle and the square.
 4. Placing different numbers in the repeated figure in the reproduction.
 5. Introducing in the reproduction a number not given, for instance, 10.

The record of Case 574 is

1. 0
2. 0
3. No
4. No
5. No

TEST IX. CROSS LINE TEST (A)

This test and Test X were first suggested for our use by Dr. D. P. MacMillan. We later found that they had the historically interesting use which is described in Test XI.

These three tests are especially noteworthy and valuable because their correct performance seems to demand mental powers which appear strongest in the normal adult mind and which are weakest in mentality of the child type. The process involved is certainly complex. It appears to call for the power of mental representation of the model, together with the ability to analyze out its parts and recall by visual memory and, perhaps, by a definite logical process the numbers corresponding to the parts. This one is simplest of the three.

The procedure we have found most useful in giving the test is as follows:—

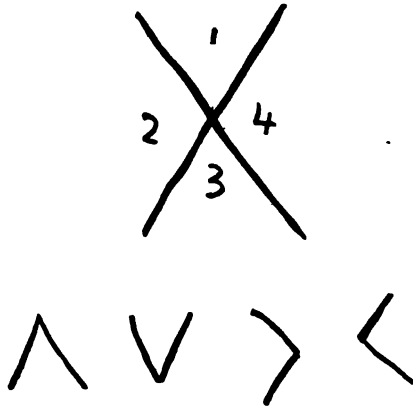


FIG. 14

The cross lines represented in Fig. 14 are drawn on a good sized sheet of paper in front of the subject, the investigator, at the same time, calling his attention to the fact that the whole figure is made up of parts or compartments. Then the figures are placed in the spaces, emphasis being laid upon the order in which the numbers are placed with regard to the figure. This having been done while the subject has had a good chance to look at the model, it is turned over and the different angles representing the different spaces of the main figure are drawn one at a time and not in regular order. As each part is drawn the subject is to tell what number belongs in it before the next part is drawn. If there are any mistakes,

he is allowed to draw and number the whole figure for himself and try again and so on until, if necessary, three reproductions have been made. He is shown any errors in his own drawing. Twelve year old children rarely fail in this test which serves substantially as a practice test for X, B.

Scoring; The record sheets themselves are, of course, direct evidence of the work done, but for a final score the following points are to be checked if positive.

1. Correct in first reproduction. (Where errors are made but corrected without assistance indicate by c.)
2. Some wrong in first reproduction, but these errors explained by arrangement of numbers as subject remembered them.
3. Correct after first drawing by self, that is in second reproduction.
4. Correct after second drawing by self, that is in third reproduction.
5. Errors made in first reproduction.
6. Errors made in second reproduction.
7. Errors made in third reproduction.
8. Failure. The reproduction where it occurred being indicated by an exponent.
9. Failure. Inability to comprehend the idea after careful explanation and several illustrations.
10. Numbers incorrectly arranged in own drawing and corrected for him. Which drawing indicated by an exponent.
11. Repetition of the same number in different situations.
12. Addition of a number not in the original drawing.
13. Inability to draw the figure correctly from memory.

Record of Case 574 is:

1. Yes.

TEST X. CROSS LINE TEST (B)

The general idea of this test will be readily understood by reading what we have said under Test IX. The main purpose is the same, but the results obtained have been found considerably more interesting and instructive on account of the greater complexity of the figure.

The procedure in giving this test has only slight variations from the preceding one. After the explanation is made and the numbers filled in as shown in Fig. 15, one element of the large figure is drawn, (always the seventh space, as shown in the illustration) and the subject is asked, "What number goes into it?" This is to make sure that the subject understands the problem. Having been given a good chance to look at the model for a moment and emphasis having been placed on the order in which the numbers are written in the

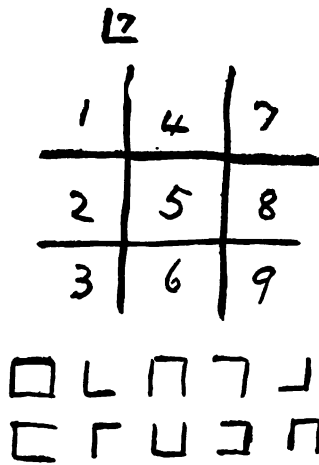


FIG. 15

spaces of the main figure, the model is turned over out of sight and the elements are drawn one by one, but not in numerical order, as the subject tells what number belongs in each. We are now trying to bring in the factor of encouragement by offering the easier elements first, namely, the fifth followed by the seventh, and then proceeding at random. If there is a failure, a chance for reproduction and retrieval is given as in the previous test. As many retrials as desirable, may be made.

On account of the readily ascertained differences in performance between bright subjects and dull subjects, we have come to regard the test as extremely valuable. About half of our twelve year old children have so far done this correctly on first trial.

Scoring: Again, on this, the original record is its own evidence of the performance, but for final registration the following points may be checked:

1. Correct in first reproduction. (Where errors are made but corrected without assistance indicate by c.)
2. Some wrong, in first reproduction, but these errors explained by arrangement of numbers as subject remembered them.
3. Correct after first drawing by self, that is in second reproduction.
4. Correct after second drawing by self, that is in third reproduction.
5. Correct after third drawing by self, that is in fourth reproduction.
6. Two numbers interchanged in first reproduction.
7. Several wrong in first reproduction. (Error in center square designated by c.)
8. Two numbers interchanged in second reproduction.
9. Several wrong in second reproduction. (Error in center square designated by c.)
10. Two numbers interchanged in third reproduction.
11. Several wrong in third reproduction. (Error in center square designated by c.)
12. Two numbers interchanged in fourth reproduction.
13. Several wrong in fourth reproduction. (Error in center square designated by c.)
14. Failure. The reproduction where it occurred being indicated by an exponent.
15. Failure. Inability to comprehend the idea after careful explanation and several illustrations.
16. Numbers incorrectly arranged in own drawing and corrected for him. (Which drawing indicated by an exponent.)
17. Repetition of the same number in different situations.
18. Addition of a number not in the original drawing.
19. Inability to draw the figure correctly from memory.

Record of case 574 is:

1. Yes (c) — prompt.

TEST XI. CODE TEST

A code that was used for secret correspondence during the civil war was communicated to us indirectly from an old army officer. It at once appeared to be the parent of the two foregoing tests, as will be seen by the accompanying illustration, Fig. 16. The code was quickly appreciated by us as a worthy member of our series. While its elementary parts are the same as those of the foregoing two tests, the task of working up a code sentence without copy introduces

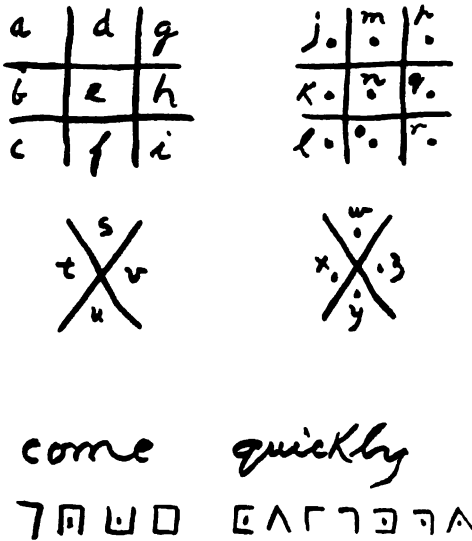


FIG. 16

a necessity for close attention and steadiness of purpose which is not equalled in any of our other work. In this test no object of interest is held in the hands, there is nothing to look at that bears on the task and the investigator is not offering the same stimulus to attention which obtains in the giving of the previous two tests. The scheme of the test is easily understood, especially if the other two tests have been previously given. The second of each of the two main figures is like the first except that each element contains a dot.

This gives twenty six different elements or spaces each to contain one letter of the alphabet. The elements and the order of the letters with regard to them being established and made plain, it becomes an easy matter for a bright subject to recall the general scheme of the code and to work out its parts in his own mind and to write a code word or sentence. Older children have been fairly fascinated by this interesting test and some have been able to write the prescribed sentence with little hesitation. We have used the words, "Come quickly" in the test partly on account of appropriateness of the phrase in connection with a war time code, but mostly because the words contain letters taken from all the main figures.

Scoring: This, again, will have to be developed later since we have been using this test for only a few months and on a comparatively small number of subjects. Types of failure or other points for scoring will have to be worked out. For the present we use the following:

1. Number of errors.
2. Errors through omission of a dot.
3. Errors where symbol given is that preceding or following the correct one.
4. Noticeably good or poor attention.

Record of Case 574 is:

1. I
2. O
3. O
4. Good. (Kept steadily at job.)

TEST XII. MEMORY FROM VISUAL VERBAL PRESENTATION

We have found that for purposes of memorizing, at least for city children, a passage about a fire offers great interest. On this suggestion, Miss Clara Schmitt wrote and first used for us the following passage which for scoring may be divided into twenty details.

If a man finds that the house is on fire | he should first look to see if it is a large fire. | If it is a small one | he should quickly pour water on it | or smother it. | But if it is large | he should run to the fire alarm box | calling out 'fire' | to the other people in the house. | Then he should go back | and help old or sick people | and little children | to escape from the burning building. | When all the people are out, | if there is time, | he may save valuable things | such as money or jewelry. | Then when the fire engine comes he | may help keep the crowds of curious people out of the way | so that the firemen may work more easily.

The nature of the test is explained to the subject and he is asked to read aloud the first sentence to see if he is capable of doing so. Then he is told to read it to himself very carefully once because we are going to ask him to tell all about it. We tell him, "We don't care if you don't say exactly the same words, but just tell us about everything it says and give everything in the right order." It is not difficult to take down as a matter of record the exact words given in response.

The significance of this as a memory test is great, but the subject's ability to read and to understand the words read must always be taken into account. By our very instructions we imply the fact that we do not count verbal accuracy in the same category of value as the recall of the ideas and logical sequence.

- Scoring:*
1. Number of details.
 - (a) Authentic.
 - (b) Imagined.
 - (c) Erroneous.
 2. Verbal Accuracy.
 - (a) Complete.
 - (b) Partial.
 - (c) No attempt at verbal accuracy.
 3. Logical Sequence.
 - (a) Logical sequence, apparent appreciation of.
 - (b) Recital of inconsequential details.

Record of Case 574 is;

(Reads passage very rapidly to himself.) "If a man finds that his house is on fire first he should find if it is a big fire.

If not, pour water over it. If it is a big fire he should go to the fire alarm box and on the way out call to people, 'fire.' If there are old or sick people he should go back and help them out. If time then should save valuables and jewelry. Next when firemen come should keep crowds back so firemen can work.

1. (a) 17
(b) 0
(c) 0
2. (a)
(b) Yes
(c)
3. (a) Yes.
(b)

TEST XIII. MEMORY FROM AUDITORY VERBAL PRESENTATION

The scope and plan of this memory test was suggested by Professor Thorndike. We have written a definitive bit of exposition about a theme which has a special child interest. For purposes of scoring it may be divided into twelve details.

"If a sailor | on the ocean | is shipwrecked | in a wild country, | he must first look for water to drink, | then he must find a place to sleep | where wild animals can't get at him, | and after that he can take time to look for food, | but he must be careful not to eat poisonous berries or fruit. | Next he had better hunt for other people on the land | and put up a flag | to stop ships which may be going by."

One says to the subject, "I am going to read a story to you four times and then ask you what I said. I don't care if you don't give me the exact words, but tell me as nearly as you can, all the things I said to you and in the same order." It is then read four times clearly and with varying rapidity and emphasis. We have tried reading this passage a less number of times, but have always been dissatisfied with the results and have come back to Thorndike's original suggestion. This and the preceding test are, of course, tests for memory, but in as much as they are passages which may

have mental representations in pictures, they are not necessarily simply verbal memory tests. They partake of the complicated nature of many memory processes.

The results seem to be worth most if calculated in terms of how the logical sequence is given, how many details are recalled and with what verbal accuracy, or transposition into the child's vernacular. As would be expected by anyone familiar with the capabilities of children, we have sometimes gotten almost verbally accurate results from those of twelve years and younger. Without much difficulty we take a verbatim record of the response.

- Scoring:*
1. Number of details.
 - (a) Authentic.
 - (b) Imagined—details added.
 - (c) Erroneous—details misinterpreted.
 2. Verbal Accuracy.
 - (a) Complete.
 - (b) Partial.
 - (c) No attempt at verbal accuracy.
 3. Logical Sequence.
 - (a) Apparent appreciation of logical sequence.
 - (b) Recital of heterogeneous details.

The record on Case 574 is:

"If a sailor is shipwrecked on some island he should first look for water to drink, next find some place to sleep where wild animals can't get at him and later look for food to eat, but he must watch out not to eat poison berries or things which would poison him. Next look for people and put up a flag that shall stop all ships that go by the island."

1. (a) 10
(b) 0
(c) 1
2. (a)
(b) +
(c)
3. (a) Yes.
(b)

TEST XIV. INSTRUCTION BOX

We have long felt the need of a test which would specifically bring out the capacity which a subject might or might not have for following instructions. Sometimes such information has seemed very desirable in connection with the necessity for estimating the subject's probable capacity for holding a position. Such a test is a distinct step towards vocational diagnosis, often a most desirable part of psychopathic work in connection with the court. Of course, others of our tests bring out to some smaller extent the power to understand and carry out directions which are given, but at the instigation of Professor Jastrow, we have recently decided to make a test especially directed to this point.

The finished box is very slightly modified from the ingenious original plan designed by Mr. Joseph W. Hayes. In order to increase, when necessary, the difficulty of the test we have added the combination lock.

The make-up of the box can be very readily understood from the following description with reference to the illustrations, Figs. 17 and 18. Its dimensions are about 6 x $5\frac{1}{2}$ x $3\frac{1}{2}$ inches and it may, of course, be constructed of any suitable wood. Fig. 17 represents the exterior of the finished box, with one side painted white and a white mark on the 3 x $2\frac{1}{2}$ inch door plainly to designate the stopping place for the numbers on the combination dial. Fig. 18 represents an inside view of the front cover of the box purposely taken before the lock was put on, in order to show more clearly the mechanism. The other sides and the bottom of the box are perfectly plain with the exception that there are small notches through which the handles project, these latter being on a lower level than the cover itself when it is screwed down in place. The handles and arms are all of wood except the small metal bar. The door opens inward and is held shut by a wooden latch, which can be turned out of the way by means of a knob on the front of the box. This latch is however held in position by a small metal bar swinging on a pivot and having its free end braced against the latch. This bar is in its turn held braced

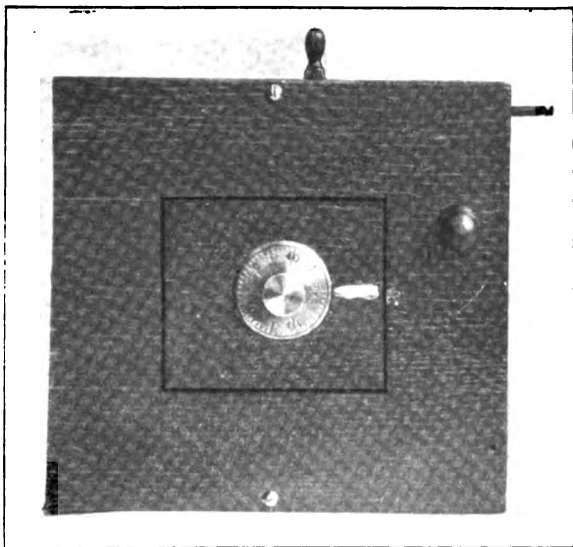


FIG. 17

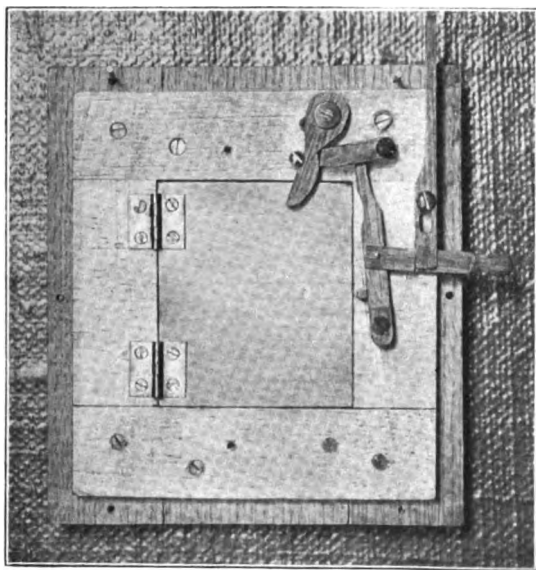


FIG. 18

by means of the cross-bar of a T-shaped catch. The handle of this latter contrivance is held firm by means of a second handle running perpendicular to the first and fitting into a notch in its surface. It is necessary in order to open the door to make the following steps in the following order. The above mentioned second handle (projecting outside of the box), must first be pulled out as far as it will go, thus freeing the other handle, (also protruding from the box), which can now in its turn be pulled out, releasing thus the pressure of the cross-piece upon the metal bar. If the box is now turned on one end (which is painted white on the outside), this bar will swing down by its own weight, freeing the latch which can now be turned out of the way of the door by means of the knob outside. This door will now be free to open except for the bolt on the combination lock. The combination itself can be set to any desired figures. We have found it necessary to avoid too great difficulty and have adjusted the two tumbler lock so as to have the numbers in such simple relation that it is not required to make turns of the dial past a specified number. By taking the tumblers apart we were able to adjust the lock so that it is only necessary, for instance, to turn to the right to 50, then to the left to 49 and then to the right till the bolt pulls back. The reader will understand that Fig. 18 shows the inside of the box without the combination lock, which when in place takes up considerable room on the door. The lock is one of the smallest and simplest made by the Yale Lock Company.

The method of procedure is simple. The child is told that this is a box which can be easily opened, if he follows out exactly what you tell him. We present it to him in the position shown in Fig. 17 and then by word and gesture pointing to the given parts so that there can be no lack of understanding, we specify slowly each step. For instance, one may say, "First you pull out the handle on this side, so, then you pull out this one on top, then you turn the box up on the white side, like this, then you push this knob as far as it will go over in this direction and then you turn the dial to the right in this way, so that the number 50 comes exactly to this white mark.

Then turn it to the left till you come to exactly 49 and then turn to the right again until you hear the bolt pull back. Then you can push the door open."

It will be found a point of practical common sense, if the subject fails at the first trial, to allow him to make a second attempt, or even a third or fourth, the investigator repeating the directions, if he is not able to correct his own errors. The combination of instructions by word of mouth and by manual demonstration is much the same as the subject would have offered to him in learning office work, or to use a machine, or in many other forms of employment. The test never fails to evoke interest. We know it is not too difficult for bright children, but if necessary the combination lock need not be set and then the door can be opened at the end of four sequential events. The performance of the test is singularly independent of the language factor, which is such a deterrent in the examination of children in our cosmopolitan cities. As an example, it may be stated that a Greek boy, 15 years old, who has only been one year in this country, and who has very poor command of English, successfully carried through all the steps in 45 seconds.

Scoring: We have not had this test long enough to be able to specify all the types of error that may be made, but scoring is obviously a matter of registration of successes and failures of each step enumerated serially, plus the time involved. For instance, one subject may do the whole thing successfully in 45 seconds, another may do only the first three steps correctly and another may succeed in doing the first four steps correctly and then blunder for a time with the combination lock until he is able to correct his own errors.

Record of Case 574 is:

Time 45". Every step carefully, quickly and correctly done.

TEST XV. ASSOCIATION OF VERBAL OPPOSITES—ANTONYMS

This is a well known test. The particular list of words we use is slightly modified from one of Thorndike's lists, and seems well suited to our purpose, some words being more difficult than others.

good	loud	dead	war
outside	black	rich	empty
quick	light	sick	many
tall	happy	glad	above
big	cheap	thin	friend

With stop watch in hand, the investigator must make quite plain, with the help of a few trial words, just what is expected in the way of answers, e.g., "When I say a word you must tell me just as quickly as you can the word that means the other thing from it, the opposite to it. Now if I say hot, what would you say?" After evidence that the test is comprehended the given list of words is gone through with at once and rapidly. In considering the results for estimation of the individual, one must take into account any foreign language factor and the amount and kind of schooling received. These are of obvious importance and may count for as much as native ability. On account of the several environmental factors which tend to beget poor results in many cases, it has seemed to us that positive findings in the way of rapidity and accuracy were more often of importance than negative ones.

We have found many twelve year old children who give correct opposites in the average time of from one and a half to three and a half seconds and who frequently get through the list with not more than one failure and one or two errors,—sometimes indeed, the score in these respects being perfect.

- Scoring:*
1. Average time of words given correctly.
 2. Number of uncorrected errors.
 3. Number of failures—longer than 10 seconds.
 4. Failure due to lack of knowledge, rather than slowness of association time.
 5. Foreign language a factor.

The record of Case 574 is;

I	I	I	I
1½	½	1½	5
1½	½	I	2
7	1½	I	I
I	1½	I	2
<hr/>			
			34

1. 1'7"
2. 0
3. 0
4. No
5. No

TEST XVI. MOTOR COÖRDINATION TEST

The method we use in this is one suggested to us by Professor Whipple. The test is intended to give some estimate of the motor coördinative power both for accuracy and rapidity. Of course, as associated elements, there always comes in the power of attention and the general mental factors which enter into almost any test. A printed rectangular form is used, divided into 150 half inch squares as shown in Fig. 19. (Black lines on white paper are used.) The subject is instructed to rest his arm on the table and hold the pencil straight up and down or nearly so, thus placing himself in the most favorable position. He is shown how to tap once in each square, beginning at the upper left hand corner and running along the line that has ten squares in it and reversing direction upon coming to the end of the line, on account of the time saving element. He is then told never to touch a line or miss a square. It is emphasized that this is a rapidity test. One must be careful to state that no effort need be made to leave a pencil mark in a square—a mere tap is sufficient. Start is made at the signal, 'Go', and the signal to end is the word, 'Stop', given sharply at the end of thirty seconds. The test is repeated immediately for comparative purposes, using the other end of the rectangle. Errors can be counted as the test is being made.

Perhaps more lack of willingness to do their best is shown here than in any other test, but after all, that factor with the class of young people we examine has very rarely to be contended with. Lack of control from sheer nervousness is sometimes encountered and often curious irregularities are

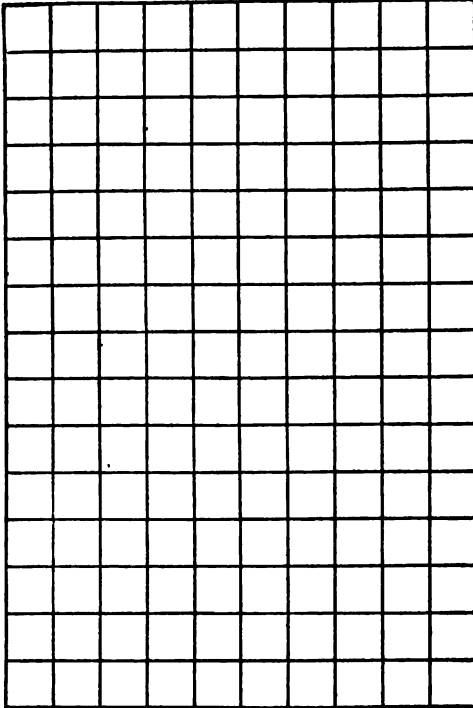


FIG. 19

obtained from the mentally unbalanced. Our 12 year olds frequently do from 65 to 75 in 30 seconds, with 0 to 4 errors.

- Scoring;*
1. Number of squares tapped in 30 seconds on each of two trials.
 2. Number of errors in each trial (including skipping a square, touching a line, or making more than one dot in a single square).

Record of Case 574 is;

1. (1) 86
(2) 92
2. (1) 0
(2) 0

TEST XVII. WRITING

For the purpose of this test we ask the subject to write a short standard sentence and his name. For estimating, it seems wise to us to have average samples which have been written by pupils of the grade which the subject has reached. These will be easily obtained from teachers.

- Scoring:* (1) Can't write.
(a) Insufficient schooling.
(b) Ordinarily sufficient schooling.
(2) Normal, poor, or good for grade. (Recorded by N., P., or G.).
(3) Slow or fast (Recorded by S. or F.).

Record of Case 574 is;

1. —
2. N.
3. F.

TEST XVIII. ARITHMETIC

The test to be assigned in number work may be based on the work of the grade in which the subject last was.

- Scoring:* (1) Knowledge of method. (Recorded by + or —.)
(2) Accuracy. (Recorded by + or —)
(3) Speed. (Recorded by + or —)

It may be possible later to amplify these scores by a study of the types of errors which are made.

Record of Case 574 is;

- (1) Partial
- (2) +
- (3) +

TEST XIX. READING

For the purposes of this test a passage should be selected which is suited in difficulty to the grade which the child has reached in school. Such passages may be readily obtained from teachers.

- Scoring:* (1) Can't read.
- (a) Foreign language a factor.
 - (b) Insufficient schooling.
 - (c) Ordinarily sufficient schooling.
- (2) Reads monosyllables only.
- (3) Fails on longer words.
- (4) Normally rapid and well for grade.
- (5) Noticeably fluent and expressive.

Record of Case 574 is;

- 1. —
- 2. —
- 3. —
- 4. Yes, plus.
- 5. —

TEST XX. CHECKERS

A test which may gauge the subject's powers of foresight is undoubtedly of value for our work. Indeed the whole possible relationship of foresightedness to moral behavior is most interesting. Fortunately in the game of checkers we have an easy test which may show some of the mental quality mentioned. However, it is only fair to apply the test with those subjects who know the game and who have played it sufficiently to be thoroughly familiar with it. It seems fairly clear that under these circumstances the results obtained, at least when very good or when very bad, must be of considerable significance for estimation of the sort of foresightedness which the game calls for. Unless one knows certainly that the subject has had good instruction and plenty of practice, negative results should not count for much. We find quite a few youngsters, especially from institutions, who have

played a good deal and once in a while it is possible to get hold of even a 12 year old boy who plays a distinctly good game.

Scoring: 1. Misses more than three advantageous chances to take men.

2. Careful, but not noticeably foresighted game.
3. Foresighted game.

Record for Case 574 is:

Said he had played much.

1. Yes.
2. —
3. —

TEST XXI. REACTIONS TO MORAL QUESTIONS

The use of this test is altogether due to the suggestion of Professor F. C. Sharp, who has used the same method and indeed the same questions extensively in a research of his own.

This is an interesting test from several standpoints. In the first place, with it one can gauge somewhat the powers of intellectual comprehension of a moral situation as expressed in language. Where an opinion is expressed clearly and definitely about the nature of the action in the story, or where there is maintained a strong idea about what should be done in like circumstances, then one may obtain some workings of the individual's consciousness upon ethical lines.

The test needs almost no introduction. We say, "I am going to tell you a story and you tell me what you think about it." Then the problem is at once narrated, if necessary, in the vernacular of the individual. Such explanation as may be needful for the correct understanding of the story is always given if required, but there is little trouble about this matter when the gist of the problem is outlined in something like the straight-forward statement made by Sharp. Naturally, of course, one has, for the younger subjects, to make the statement somewhat more personal and elaborate than given in the

mere outline. For instance, in the second problem, we find it more efficacious to say, "What would you have done if you had been captain of the village?" A short amount of cross examination we have often found necessary in order to find out the child's real opinion and also to ascertain if he would change this, and to see if he possesses realization of the complexities the situation presents. It is usually easy to record the answers verbatim or as memoranda.

Professor Sharp's concise statement of the problems is something as follows:

(1) In a Russian city last year there lived a man who could get no work. He had for a neighbor a sick widow with two little children, who were starving. The poor man took some bread that did not belong to him from a baker's shop, because he could get it in no other way and gave it to the widow and her children. Did he do right or wrong?

(2) A settlement was once besieged by a large body of Indians because the chief thought that one of the white men had done him an injury, though he really had not done so. The chief sent word to the captain of the village that if the man was given up to him he would go away, but if not he would burn the village and kill the people. The captain and the people knew that if the Indians attacked them they would be very likely to capture the settlement and, at least, would kill a good many. They also knew that their fellow citizen was innocent and that to give him up meant torture and death for him.

What was the right thing for the captain of the village to do and why?

What would you have done if you had been captain?

We have long since concluded that for our purposes, one of the best points connected with this test is that it offers a good opportunity for estimation of the subject's power of intellectual comprehension. Beyond this, we have found in many cases the answers given apparently mean very little with regard to the subject's own moral nature. Certainly we would hesitate much to draw therefrom inferences about

the possibilities of his conduct. But occasionally we have received such striking reactions that we have felt we had gained a considerable insight into the character of the individual; for instance, if he steadily dwells upon the necessity for bravery or for self-sacrifice in answer to the second problem. We must then form quite a different impression about his moral make-up than if his statement expressly stands for selfishness. Expressions such as "Take care of yourself every time, that's what I say," "Get out of it yourself, if you can, and let the other fellows look out for themselves" mean something quite different from, "It's a captain's duty to take care of his people. If that man was a very valuable man, the captain should go out and offer himself to take the man's place." Then there is a type of mind, we discover, which will venture an opinion wavering with every little breath of suggestion from us. That seems significant in contradistinction to the sturdiness of others who maintain steadily their first point of view.

The idea has been offered us that the first question might contain a thought which would be harmful to children, but we concur in the opinion expressed by Binet that children are by no means as susceptible to the influence of little stories of morbidity and human peculiarities as people generally believe. We have been interested time and again in noting the excessive reaction against the action of the poor man who steals to give to starving children by the very youngsters who are themselves chronic little thieves. However, on the whole, the first problem seems decidedly the less valuable and we frequently omit it.

Scoring; So far, we conceive the best points to score from the investigator's full memoranda are the following:

1. Apparently a more or less haphazard reply given at first, followed by
 - (a) An unthinking contradiction in response to your suggestions.
 - (b) A childish repetition of his original statement without considering your arguments.
2. Apparent comprehension of the moral situation.

Record of Case 574 is:

- I. Done wrong cause he stole. Done right if he stole for starving people. He could have asked for it. Yes, think he did wrong. (Would you have done wrong then if in his place?) I suppose so. (Slow. Shows suspicion in answering this.)
- II. No, I'd have fought—as long as the man hadn't done wrong. Even if many were killed and village burned still will be better than to give up innocent man. (Persists in this.)
- I.
 1. (a) Yes.
 - (b) No.
 2. No.
- II.
 1. No.
 2. Yes. Decidedly good comprehension.

TEST XXII. INFORMATION

The following list of questions to be asked has been gotten out not only for the purpose of eliciting the actual information possessed by the individual, but also in order to get at what his opportunities have been in general environment and in school life. Through this inquiry one can readily get some indication of the amusements, occupations and aspirations of the subject. (Perhaps it is hardly necessary to say that when special points of character or mental peculiarity are involved, a very much more extensive inquiry about motives, and interests and opportunities is always instituted by us.) Questions may be asked and answers filled out to any extent deemed advisable, but for our purposes the following list has been made up with the help of experienced teachers and after much consideration of both the healthy and unhealthy interests fostered by the every-day, city life of young people. The grouping and the purpose of the questions is apparent to the reader. The questions are verbally presented and the answers may be rapidly filled in as memoranda by the investigator. Under this procedure the whole list takes up only a short time.

Read books?	What makes light of moon?
What ones?	Flour made from?
From library?	Mortar made from?
How many at home?	Where do coal, oranges, cattle etc. come from?
Read newspapers? Which?	What do you expect to be when grown up?
What parts?	
What has interested you lately?	What do you want to be?
Ask about recent murder or burglary.	Jobs easiest? Most dangerous?
Ask about recent disaster, etc.	Father belong to trade union?
Who is President U. S.?	What do you think of them?
President before him?	Play what out-door games?
First President?	Cards? What games?
What celebration is 4th of July?	Have you tools?
Abraham Lincoln? Who? When?	What things ever made?
What lake by Chicago?	Pet animals?
Five Great Lakes?	Collections? Car transfers, buttons, pictures, stones, shells, stamps, etc.
Largest city in America?	Dolls?
Capitol—where President lives?	Cooking? What?
Jesse James? Who? Where hear of him?	Fancy work?
What does cow-boy do?	Sewing? Make own clothes?
Best base-ball team last year?	Athletics? Where? What? How good?
Champion prize fighter?	Parties? What? With whom?
Champion wrestler?	Theatres? With whom?
Been to many plays?	Nickel shows often? With whom?
Best one you ever saw?	Games at home?
What is steam? How works an engine?	Classes; singing etc?
Electricity? What does it do?	Music? Instruction?
What heart beats for?	

TENTATIVE CLASSIFICATION.

We have had surprisingly little trouble with the classification of cases in the scale of mental ability as given below. Perhaps that is because no *a priori* standpoint was taken and divisions were not forced. We followed the scheme of waiting till about 250 cases had been seen and then these were sorted into the places into which they seemed themselves to fall. This system quickly led to the classification which, with a little more exact definition and slight modifications, has proven well its practicability. It is only rarely that we feel

unable to decide between two classes in the following schedule, although we still conceive it to be entirely tentative.

(a) Considerably above ordinary in ability and information—the latter estimated with reference to age and social advantages.

(b) Ordinary in ability and information—the latter estimated with reference to age and social advantages.

(c) Native ability fair and formal educational advantages fair or good, but very poorly informed.

(d) Native ability fair and formal educational advantages fair or good.

(e) Native ability distinctly good, but formal educational advantages poor.

(f) Native ability fair and formal educational advantages poor.

(g) Native ability poor and formal educational advantages poor.

(h) Native ability poor and formal educational advantages good or fair.

(i) Dull from known physical causes, including epilepsy.

(j) Subnormal mentality—considerably more educability than the feeble-minded.

(k) Feeble-minded, (Moron).

(l) Imbecile.

(m) Psychoses.

(Estimation 'formal educational advantages poor' includes no implication of cause, it may be due to chronic truancy or to faulty environment.)

Over and beyond this general classification there is certainly often much else that may usefully be said about the subject from a psychological standpoint. During the course of work with him other points of inestimable value in diagnosis may be discovered. Adventitiously we may find individuals markedly lacking in general powers of attention, emotionally disturbed by special circumstances, wanting in normal interests of their age and sex, tremendously defective in veracity and so on. Then, on the positive side, we have found instances with important unsuspected abilities

for art, mechanics or language, or with highly developed standards of bravery, honor and even honesty—any of which points may well seem determinative for prognosis, if the individual can be placed under favorable conditions. Some discovery of aptitude or inaptitude, such as constructive ability or deficiency, is most valuable from the standpoint of vocational diagnosis which one is often called on to make. As an instance of what effort may be wasted if the case is not studied before treatment, the following is instructive; we lately examined a boy who had much difficulty in manipulating our constructive tests and who showed no interest in any way in mechanical contrivances. This notwithstanding the fact that he had been two years at the state manual training school. That institution had given him up as a bad job, after he had run away some four or five times, out of sheer lack of interest in the special opportunities presented to him. Now if he had been tested before being assigned to this school, the outcome might have been foreseen, so obviously unadapted was he to the type of occupation forced on him. A striking proof of the reverse we have seen in the instance of a 15-year-old orphan boy, notorious for deeds so peculiarly vicious that he was said to be insane at times. He had committed one decidedly serious crime. Badly handicapped by partial deafness from bilateral middle ear disease and extremely refractory to rules of conduct the boy was, even in reformatory institutions, considered a distinctly undesirable person. Finally, while he was being studied by us, it was discovered that he had considerable talent for drawing. Then it followed that with encouragement, instruction and the placing of art materials in his hands, his criminal deeds and 'insane spells' at once dropped away from him like shackles. This opportunity for self expression has given him just the one satisfactory interest in life that he has ever possessed.

It is easy to see that all the psychological findings can not be expressed in terms of any classification. We find it best always to write up in detail special impressions directly after the examination—impressions that may well be modified as the case is followed and further observation is made.

It is, of course, to be clearly understood that where special problems are involved, for instance, in cases of suspected dementia praecox, the above tests are supplemented by others especially adapted.

TABULAR SCORING RECORD

We append herewith a tabular scoring-form. For the practical purpose of comparing results of tests with one another in the same individual or similar tests in different individuals of the same age a businesslike arrangement is essential. To Mr. W. F. Dummer is due the credit of having developed the following ledger form, which by a system of colored lines makes tabulation and future deciphering particularly easy.

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PART I. INTRODUCTION

THE PLAN AND SCOPE OF THE EXPERIMENTS

The experiments described in the following pages were designed to discover whether the differences which appear among individuals, in certain simple acts of the attention, are indicative of typical traits of the attention, or whether they are fortuitous and unrelated. Individual differences in attention are easily found and have been frequently described, but few efforts have been made to determine what traits go together. Popular judgments on such questions are very common. The unconscious sorting out of the bright and the dull, the absent-minded and the alert and many other contrasts, which everyone is constantly making in his daily relations with other people, is a recognition of differences in attention and possible groupings of them for practical purposes. Teachers are very prone to generalize in the estimates of their students, and undoubtedly many of their shrewd discriminations would be borne out by laboratory tests. Though it is exceedingly probable that a great many convictions which have grown up in the classrooms are not based on a sufficient or accurate induction. Thus, the common 'rules' that a fast reader remembers less than a slow one, or that the student who learns quickly forgets more easily than the plodder are specimens of the popular correlating which would quickly lose their force under careful observation or experiment.

In endeavoring to detect typical traits of the attention the series of experiments to be described were planned to consider several characteristic attentional acts and to ascertain the individual differences which appeared in each. These differences were then studied and correlations sought by a mathematical formula. Throughout the entire work each

experiment has been reduced to as simple terms as possible, and every effort was made to make the measurements accurate.

Of course, in all such work the larger the number of individuals studied the better. It is not practicable, however, in a psychological laboratory to examine great numbers and it is impossible in work of the kind in hand to go outside for material, as the apparatus is not easily transported. In all about twenty-five people participated in the experiments. From these it should be evident whether some traits of attention are indicative of types or not; though a larger number must be examined before any far reaching conclusions are attempted. This further study is the more desirable as the subjects were university students and had had from four to seven years of college work behind them; which, of course, would tend to give them similar habits of mind. Far greater diversities in concentration, for example, would probably be found among an equal number of men chosen at random in the street. This uniformity of training is a disadvantage in studies of natural differences, but the advantage of having college-bred subjects in some measure compensates for it. For much of the work calls for introspection, which could hardly be obtained from those who had no such training.

The number, and therefore, the character of the experiments were limited by the conditions of the experimentation. For example; no work on the influence of fatigue upon attention was attempted, when it became apparent that the one hour allotted each experiment would not suffice to induce fatigue in all the subjects; nor was it practicable to find how the personal interests and habits of thought of the various observers affected attention. Questions of involuntary attention and of fluctuations were not studied; not because they are not of importance in finding Types, but because the difficulties to be overcome in preparing conditions were too time-consuming to admit them. To obtain material which would permit accurate comparison of one subject's results with those of each of the others it was thought advisable to limit the field to tachistoscopic work as far as possible. Differences which creep in when the apparatus is frequently

changed, were thus obviated. This restricted the field to visual work, in large measure, but allowed ample scope for individual differences to show themselves. In the tests for 'span' different classes of objects were exposed, which called for different attitudes of attention in the subjects and showed individual differences clearly. So in the other tachistoscopic tests; a comparatively wide variety of experiments was found practical.

The scope of the work, therefore, is limited to groups of university students, each group studied during one college year in one hour periods. The apparatus largely restricts the work to attention in visual perception.

The plan of the experiments under such conditions shaped itself into a study, first, of the characteristic span of attention of each subject in observing several classes of objects, then, the ability to concentrate upon certain things and to inhibit others, this led into an examination of the differences in the faculty of turning the attention from one class of interests to another and the quickness with which this could be done. Throughout the work the experimenter noticed differences in the records of subjects due to the memory factor. These were studied with a view to detecting a possible correlation between memory and attention. Finally, the question of the relation of memory types to types of attention concludes the experimentation. The results are given in each chapter in tables which show the relative capacities of the subjects for that experiment. This determines the subject's rankings which are used in the correlation tables. From the correlations the conclusions are drawn concerning the possibilities of Types of Attention; in the experiments used.

HISTORICAL SETTING

There is very little literature upon Types of Attention. Scattered references may be found in recent studies, but no thoroughgoing effort has been made to find actual typical characteristics in the individual differences in attention. The nearest approach to the experiments in hand are the

tests made for individual differences in several faculties. Some of these we shall consider later.

While the great body of psychological literature is silent upon the matter of Types of Attention it nevertheless bears witness to them in a very unique way. For many writers upon philosophical and psychological, or near-psychological, topics since Plato's time have touched upon the activities of attention, and their opinions give an interesting insight into their own mental processes. We find throughout the history of the theories of attention two general classes of opinion¹. The one holds that the attention is more or less subjected to the play of stimuli upon the mind, that the inward direction of one's course of thought is to be found in the influence of his surroundings. The other view emphasizes the influence of the mind upon the activity of attention regardless of the external world. It magnifies the domination of a will or a self. Of course the general philosophy of a thinker will modify his conception of each chapter in psychology; but, in spite of that, the disposition of his own thinking will tend to be the criterion by which he will be guided. If he is easily distracted by his surroundings, if he finds it difficult to concentrate upon intellectual problems rather than perceptual, if his attention is of a discursive nature, passing easily from one subject to another, then nothing could be more natural than the conclusion that the attention is directed by stimuli rather than by will. On the other hand, a mind which readily concentrates upon abstract themes, ignoring the passing appeals of the outer world, feeling its ability to turn now to one phase of the subject considered, now to another, without let or hindrance, is very apt to be a mind which concludes it possesses an independent control over the attention. From such experiences the two divergent types of theory, the involuntary and the voluntary, undoubtedly evolved. They are descriptive of two types of attention, insofar as they are based on intro-

¹D. Braunschweiger, *Die Lehre von der Aufmerksamkeit in der Psychologie des 18. Jahrhunderts*; L. L. Uhl, *Attention, an Historical Summary of the Discussions Concerning the Subject*.

spection, and few men are so abstract in their treatment of such matters that the evidence of their own mental life does not affect their systems of thought.

Such differences as those appearing between the positions taken by Condillac, Bonnet and Helvetius on the one hand and by Reid and Stewart on the other bespeak more than philosophic and literary differences. The natural differences of race and mental traits come into the writings and account in large measure for their attitudes on the question of attention.

It is perfectly legitimate to assume that we have the materials for two types of attention, at least, in the mental life of psychologists and philosophers. It would be a valuable study if the influence of these fundamental mental traits were traced out in the systems of philosophy which these minds found the most acceptable and congenial.

Baldwin finds five classes of modern psychologists; those who maintain that the affective element is dominant in attention, namely Horwicz and Ribot; those who find in attention an original activity of the mind; Stumpf, Ladd, Ward and Jodl; those who refer the fundamental factors of attention to intensity of sensation and perception and their psychic reinforcement, such as Bradley and Müller; those who see attention as the outcome of inhibition, Ferrier and Obersteiner; and the champions of the theory that attention is to be understood in the light of the motor factors, Baldwin, Lange, and Stout. In the last group of theories we should consider Münsterberg's 'Action theory' and the theories of McDougall and Sherrington.

Not until the time of Francis Galton do we find any consistent study of individual differences.¹ His suggestions were carried out experimentally among school children by Gilbert, Boas, Bolton and others. However, the most nearly related to Types of Attention is the work of H. Griffing *On the Development of Visual Perception and Attention* in which he tries to establish a correlation between the Span of Attention and

¹*Inquiries into the Human Faculty*. 1883.

the mental ability of the subjects. Baldwin's doctrine that the attention is as subject to ideational differences as memory and imagination, which appeared in his *Mental Development* was not based on any experimental fact. In the work of Henri et Binet, *La Psychologie Individuelle*, 1896, we have the recognition of the fact that attention may differ in typical ways among individuals. Later Binet endeavored to disprove Gilbert's correlation between mental ability and span. The work was done on *auditory* span. Binet and Vaschide made many experiments for differences in reaction-time, memory and general mental ability, but none bearing directly on our subject. The article of S. E. Sharp upon Individual Psychology gives a very good résumé of what work Binet did on attention.¹ He sought to determine the degree of attention by having his subjects cancel certain letters as they read a page. The range of attention was sought by having the subjects read aloud and write the alphabet simultaneously. Both methods are, of course, very faulty and no one should expect clear-cut results from them. In L. W. Stern's *Ueber Psychologie der individuellen Differenzen*, 1900, we have a very able treatment of Types in psychology. In attention he finds the concentration (Konzentration) and the expansive (Elastizität) features of attention indicative of two types. Also, he finds typical differences in constancy of application and the intermittency of concentration. He maintains that the psychic energy used in attention varies. This explains some of the phenomena of differences in morning and evening workers and of sleep as well. He does not attempt any large correlations between types.

Kraepelin and Cron in their work *Ueber die Messung der Auffassungsfähigkeit* made tests of the simpler mental factors and found the results far from satisfactory. This failure of correlation is rather characteristic of experiments in individual differences; witness the paucity of results from the experimentation of this character by Wissler, Thorndike and others upon students. Spearman, nevertheless, believed

¹*Amer. J. of Psychol.*, x, p. 329.

from his experiments that a correlation could be found between all forms of sensory discrimination and the more complicated intellectual activities of practical life. He sums up his results thus: "All branches of intellectual activities have in common one fundamental function (or group of functions), whereas the remaining or specific elements of the activity seem in every case to be wholly different from that of all others."

The discussions of the work of Münsterberg, Cattell, Jastrow, Calkins, and others which deal with the larger problems of Individual Differences need not engage us here. Our retrospect upon the historical feature of Types of Attention shows the need of a beginning, at least, in the literature of this subject.

APPARATUS AND SUBJECTS

The apparatus used in nearly all of the experiments was a very simple form of tachistoscope. It was fastened to a low table and leaned toward the subject, so that he could conveniently place his eye close to the shutter-opening. It was simply constructed with a base-board 36 x 14 inches which sustained supports holding a similar board above it. This upper board was arranged with an incline toward the subject. It was 16 inches above the base-board along the line farthest from the subject and 12 inches along the line next him. A frame about 3 inches high held an automatic camera shutter over an orifice in the center of this inclined upper board. Immediately beneath was a heavy grey card, parallel with this upper board, and 17 inches from the subject's eye. Upon this card the smaller cards, containing the objects to be observed, were placed. The shutter was controlled by a pneumatic device. A pendulum, regulated to traverse its amplitude twice in a second, hung beside the experimenter's chair. The experimenter was hidden from the subject by a cloth screen which was suspended by a rack on the upper board. The whole frame was covered with black card-board, except the back which was left open for light and to enable the experimenter to change the exposed objects easily.

Nothing was visible to the subject but the screen upper, board and shutter. When the shutter sprang open the white card, $7 \times 5\frac{1}{2}$ inches, which held the objects to be observed, was clearly in view. The letters used were $\frac{1}{8}$ inches high and were black. The colored letters were about $\frac{1}{2}$ inch in height, and the colored figures were from $\frac{1}{2}$ to 1 inch in height. Where the squares of color were used, they were about $\frac{3}{4}$ inch each way.

Another apparatus was used in the experiments upon visual and auditory attention which might be termed a rotary tachistoscope. It was a wooden disk 24 inches in diameter hung upon an axle, and connected by a rubber belt with a dynamo whose wheel connections admitted of several speeds. Thirty-eight words in white letters were fastened at regular intervals near the circumference. Immediately in front of the wooden disc was a black card-board disc with slits opposite the words. When these two rotated together, words flashed up before the eye and disappeared without their motion being observable. The eye was kept in position by looking through another slit card-board about 1 foot from the black card disc.

In the work at the Laboratory in Princeton a tachistoscope was constructed from two black card-board discs 50 c.m. in diameter. Each disc had a quadrant cut out of it, and they were rotated with half of the surface of one disc covering half of the surface of the other. Where the cut portions coincided the observer had a clear view of the card to be exposed. Attached to the wheel, on which one of the discs was fastened, were several copper fingers. These made and broke electric currents to magnets and bells, at the moment when the card was exposed.

The subjects participating in the first year's series of experiments were four graduate students in Harvard University, a practicing physician and a professor in psychology. In the second year three of the subjects were instructors, the others being graduate students who had had considerable laboratory experience. Two of these were women. In the third year the nine young men were all undergraduates in Princeton.

PART II. EXPERIMENTS

THE SPAN OF ATTENTION

In measuring attention we feel the need of an exact definition of what we are measuring more keenly than in any other treatment of the subject. Our work will be confusion worse confounded if we do not have a clear-cut idea of just what is being measured. Unfortunately we encounter the old difficulties, which are met in every attempt to define consciousness, those of implying the thing defined in the definition. This 'circulus in definiendo' is a very subtle snare just here. Attention is a characteristic of every mental act, we are always in the center of it, we cannot get away from it and survey its borders from without. Nevertheless, we may make clear what is meant in the present chapter by attention, with illustration and comparison.

When we look at a printed page there is always some portion of it, perhaps a word, which we see more clearly than the rest; and out beyond the margin of the page we are still conscious of objects which we see only in an imperfect way. The field of consciousness is apparently like this visual field. There is always a central point of which we are momentarily more vividly conscious than anything else. Fading gradually away from this point into vague and vaguer consciousness, is a margin of ideas, or objects, of which we are aware in some sort of mental indirect vision. This fact that consciousness always has a focal point, which reveals the momentary activity of the mind, is what is meant by the fact of attention.¹

This is expressed graphically by means of five concentric circles. The outermost circle representing unconsciousness; the next inner circle, subconsciousness; the next, diffused consciousness; the circle next to the center is called active consciousness or attention and the center circle represents apperception, which synthesizes all mental data.

Our problem is the measuring of this focal area in consciousness. That it is not anything like so simple and static

¹Angell, J. R., *Psychology*, p. 65.

a thing as the series of circles might imply goes without saying. Indeed, the mobility and elasticity of that central area are equalled only by the vagueness and uncertainty of the several grades or degrees of consciousness represented. It would seem at first sight that the problem of determining where this central clearness ends and the peripheral obscurity begins is as thankless a task as finding the line between candle-light and the darkness enveloping it. There are, fortunately, some features in the process which lend us material aid. As Wundt showed, many years ago, every process of attention has two factors, the one increases the clearness of an idea or perception, the other diminishes, by inhibition, other available impressions or memory-images. So our area of light is heightened in its clearness and our circumference of obscurity is darkened, making the margin of uncertainty between the two narrower than it first appeared, though all too broad at that. Titchener adds,

Attention, in other words, means a redistribution of clearness in consciousness, the rise of some elements and the fall of others, with an accompanying total feeling of a characteristic kind."¹

A large number of careful experiments have been made to measure this span of clearness. Usually they have sought to catch the act of attention in an instant of time and to take a sort of psychological snap-shop of its processes. This, of course, applies to attention concerned with spacial perceptions, not with temporal, and it is with the spacial aspect we shall deal. A tachistoscope is usually employed which either illuminates the field observed for a fraction of a second or exposes the field by the opening of a shutter. The observer has a fixation point in the middle of the field which obviates loss of time by eye movement, and makes measurements more exact. Wundt says:

careful introspection easily succeeds in fixating the state of consciousness at the moment the impression arrives, and in distinguishing this from the subsequent acts of memory. . . . these experiments. . . .

¹Titchener, E. B., *The Psychology of Feeling and Attention*, p. 183.

show that the scope of attention, when it is kept at its maximum intensity, remains constant only when the impressions are held apart as in the case of isolated lines, numbers, letters.¹ Six such simple impressions can, under favorable conditions, be apperceived in the same instant. As soon as the impressions are bound together in complexes the number included in the scope of attention changes.

This conclusion is borne out by the experiments of Goldscheider and Müller at Berlin². They found that for isolated lines exposed for only $\frac{1}{16}$ of a second, four or at most five could be recognized. When the lines were grouped in symmetrical forms a larger number could be apperceived. Cattell, Erdmann and Dodge corroborate these findings in their experiments upon the psychology of reading.

We may be sure, then, that the process of attention when concerned with a few unrelated impressions is unlike the process with which we are familiar in the ordinary activity of attention; where we naturally associate and assimilate the impressions and retain these synthesized groups in our memory. But we may not be sure as to just how far these so-called unrelated objects fail to be apperceived. In a few trial experiments upon this problem I quickly discovered that the ability to apperceive in groups varied in the same individual for different arrangements and varied widely in different individuals. Not only was this confusing situation present, but a more subtle difficulty presented itself in the fact that a certain amount of grouping may occur in an unconscious manner. That is, the lines vaguely suggest a box, a house, or a face, but not clearly enough to make it a matter of comment, or even of notice, unless questioned by the experimenter. In such cases the span of attention will appear large, though the cause is a purely accidental one. This occasional activity, the assimilating of impressions into larger wholes, cannot be entirely excluded from any experiment upon attention. It is one of the several disconcerting factors which we must seek to minimize. I found, however,

¹Wundt, W., *Outlines of Psychology*, Third English edition, p. 236.

²*Zeitschrift Klin. Med.*, Bd. xxiii, p. 131.

in many hundreds of experiments that the subject's introspection and the report he gave went very well hand in hand. That is, when a series of words suggested a sentence and so gave unusually large returns, or when several colors grouped themselves as shades of a fundamental color, I could detect the ease in which the report was made and so corrections could be made, or the result discarded. I do not believe that this grouping is as prevalent in those experiments where the impressions themselves are a complex such as words, colored figures, or geometrical forms, as in those simpler presentations where it is difficult to arrange mere dots and dashes so that they will not suggest some form or figure to the eye. Rather the association will arise from alliteration or rhymes in the words or a serial order in the numbers, etc. But these difficulties are in large measure obviated after a little experience and practice in arranging the material to be presented. For this, and other reasons, the experiments which I made to measure the span of attention were uniformly of longer duration in exposure, and of more varied and richer content in impression;—tachistoscopically speaking, they are less of a 'snap-shot' and more of a 'time-exposure' than those classical experiments from which we have been quoting.

The method best adapted to detect individual differences in the scope of attention must be one which enables the attentional processes to act in as normal a way as is possible in laboratory work and which gives the experimenter a comprehensive view of what these processes are. Such a method, I believe, is, at least, approximated in the work now to be described.

Three series of experiments were made extending over several months and giving each subject a sufficient number of trials to enable him to express in averages his characteristics. Any one lot of experiments, taking the subject for one hour only, would not in all probability, tell a true story. I recall one subject whose results were fully 20 per cent better at one time than at another. It was a very extreme case; but the condition of the subject, if fatigued, nervous, confused by the experiment or what-not, will affect the results of his

work. This objection is done away with by the number of experiments and the wide variations in time between them.

As Dearborn found it necessary to vary the style and subject matter in his reading tests, so I found it necessary to vary the classes of objects exposed. As we shall see later, the sensory and motor elements entering into attention vary in their proportions for words and colors. With colors a visual memory image usually persists; this seldom occurs for words. Were the tests made for one class of exposures only we should probably have errors creeping in from differences in ideational type. To escape this I have taken one series of experiments with the attention upon words, one with the attention upon colors, one with the attention upon figures and letters, and I have used the sum of these results to represent the Span of Attention for the several subjects.

It became apparent early in the work that a time exposure of three seconds was too long for many of the subjects. One thing inhibited another and there was always a feeling that much more had been attended to than could be reported. It also became as quickly apparent that a half of a second was too short a period. For in this class of experiments the eye had no fixation point and would usually catch two or three objects, but would fail to survey the entire field. It was merely a matter of where the eye chanced to rest that determined the class and number of objects. After trying several fractions of a second it seemed advisable to use exactly one second's time exposure. This enabled the subject to survey an entire card, and did not permit him to overload his mind with observations which could not be retained. It also seemed to be an opportune time for preventing the grouping of objects, such as occurs with long time exposures. Associations would occur occasionally, but not frequently. Indeed, five monosyllabic words were frequently given without the subject forming any idea of their meaning, until they were reported.

At first the subject was requested to write all that had been observed. This had the effect of driving many things out of mind for several subjects, especially those whose memory

images did not seem as clear or of as long duration as the majority of subjects. So the plan of giving a verbal report was adopted, which entirely altered the results for several subjects. More in detail the experiments were as follows:

In series No. 1, (see Table I) 50 exposures were made of 10 colors, upon the regular cards, with the regular one second exposure. The colors were shown to the subject before the experiment began, in order to familiarize the subject with the kind of color to be exposed and to learn his nomenclature for colors. All that was insisted upon was a sufficiently clear report, to make certain that the colors named had been actually perceived.

In series no. 2 (see Table II) 20 exposures were made of cards containing 5 three-letter words and five colored letters or numbers. The attention was directed to the words in order to involve the speech-motor factor in attention. If other things than words were perceived they were allowed to count in the total of things spanned by the attention. The proportion of objects perceived which were not in the class attended to is exceedingly small; as we shall see later. The totals are, therefore, a satisfactory index of span for this class of work.

In series no. 3 (see Table III) 20 exposures were made of the same class of cards as in series no. 2; but the attention was directed in this case to the colored letters and numbers. The purpose in this was to submit a class of objects to the subject's attention which were intermediate between the color-class and the word class. For it had been noted in previous experiments that the setting of the attention was introspectively different for these two classes of objects.

In Table IV the totals for these three series were given for each subject; thus, the number of colors 'A' perceived in the first series was 215, the number in the second series was 97, and in the third 206. The total number of objects attended to in the ninety experiments was 518. That is, 'A' attended to an average of 5.75 objects for each exposure. This figure or the total itself is the index of 'A' for Span in this class of experiments.

TABLE I

SUBJECTS	TOTALS	CR.	AV.	MV.
A.....	215	4	4.30	0.58
B.....	205	7	4.10	0.62
C.....	223	1½	4.46	0.62
D.....	161	9	3.22	0.55
E.....	222	3	4.44	0.44
F.....	141	10	2.82	0.40
G.....	213	5½	4.26	0.57
H.....	199	8	3.98	0.46
J.....	213	5½	4.26	0.62
K.....	223	1½	4.46	0.69

TABLE II

TOTALS	CR.	AV.	MV.
97	3	4.85	1.18
86	6	4.30	0.80
70	10	3.50	0.90
96	4	4.80	1.02
106	2	5.30	1.13
72	9	3.60	0.55
74	8	3.70	0.93
88	5	4.40	0.88
84	7	4.20	0.90
125	1	6.25	1.14

TABLE III

SUBJECTS	TOTALS	CR.	AV.	MV.
A.....	206	1	10.30	2.04
B.....	137	8	6.85	1.44
C.....	169	5	8.45	1.74
D.....	122	10	6.10	1.04
E.....	185	2	9.25	1.64
F.....	123	9	6.15	1.25
G.....	141	7	7.05	1.12
H.....	166	6	8.30	1.60
J.....	172	4	8.60	1.30
K.....	182	3	9.10	1.62

TABLE IV

TOTALS	CR.	AV.
518	2	5.75
428	7½	4.75
462	5	5.13
379	9	4.21
513	3	5.70
336	10	3.73
428	7½	4.75
453	6	5.03
469	4	5.20
530	1	5.88

C.R. = Correlation Rank.

Av. = Average.

M. V. = Mean Variation.

From these tables it appears that there are large individual differences between subjects in their span of attention. Thus, 'K' has half again as large a span as 'F'; though several have span results quite close together 'B' and 'G' having the same totals.

In the table of correlations it will be seen that Table III correlates with Tables I and II, but that Tables I and II do not correlate. This shows there is a sufficient difference between the word-class and the color-class to affect the attention quite differently. It also shows that the experiments are successful in measuring a characteristic spanning of the attention; for the first series and the third, between the perform-

ance of which several months elapsed, have the correlational coefficient of 0.48.

Another evidence of the accuracy of the experiments appears in the tables for averages and mean variations from the averages. It is not possible to find any Probable Error in these measurements for an unusually large or small result does not necessarily indicate an error of any kind. The value of the experiments would be greatly reduced, however, if each subject varied very greatly from his average, for no figure would then represent his span accurately enough to permit comparison with the others. When the averages in the tables above are compared with their mean variations it will be seen that the variation is but a small part of the average (usually a sixth, occasionally a fourth). So the average is amply representative of the subject for comparison. The average is not given in succeeding experiments as it was found that the actual totals of results give a more accurate ranking for correlations.

The correlations for the above experiments, as they compare with each other and as they compare with the other experiments of the thesis, are all given. A separate table is also given of those correlations which are sufficiently above the Probable Error, for this method of correlating, to indicate significant relations. The method of correlating is discussed in the opening paragraphs of Part III.

CONCENTRATION AND INHIBITION

Attention has been regarded as simply the focussing of consciousness, which resulted in a consequent brightening of all it encompassed without directly affecting other portions of consciousness. It has also been regarded as the processes by which all other parts of consciousness are obscured except that which is directly engaged in perception or ideation. There is surely virtue in the 'golden mean' in this case. For that sharpening between the area of clearness and its penumbra of obscurity, which was mentioned in the last chapter, is much better understood if we consider both processes as complementary factors in attention.

Probably the sharpness with which the clearness area drops away into the obscurity area differs with different individuals. The introspection of Külpe, however, appeals to me as being in close concord with the average type of attention. He sums up his views:

When we ask how the degrees of consciousness are related to one another we find not one uniform graduation from the highest to the lowest, but in most cases a fairly sharp line of distinction. Certain contents stand at the level of clear apprehension; and from them our consciousness drops away; without transition, to the level of obscure general impression, above which the other contents of time are unable to rise. And the clearer the first group, the more indistinct are all the rest.¹

Physiological psychology gives additional weight to this view.

Striking effects of concentration upon any object are frequent in the experience of everyone. There can be no doubt that we must find a physiological expression for this singleness of the object of attention and for the power of one object to banish all others from the focus of consciousness. Translated into physiological terms it means that only one of the perceptual systems of the cortical paths, consisting of one or more sub-systems of sensory areas united by higher-level paths, can be active at any one moment, that the spread of the nervous excitement through one such system somehow brings about the cessation of activity in the system active at the previous moment and prevents the activity of the other systems. Hence we need not seek for inhibitory centers in the cerebrum. Each perceptual system of arcs is an inhibitory center for every other, the activity of each system brings about as a collateral effect the inhibition of all others. . . . Though we do not know how this inhibition is brought about, it may be conceived as a drainage of the free nervous energy from the inhibited to the inhibiting system, owing to the latter becoming for the moment the path of least resistance. There is evidence that similar inhibitory effects are excited by the activity of any one group of arcs of sensory area of the cortex upon other arcs of the same area, especially in the case of the visual area.²

Such a line of demarcation between the fields of attention and non-attention enables us to determine with considerable

¹Titchener, E. B., *The Psychology of Feeling and Attention*, p. 222.

²McDougall, W., *Physiological Psychology*, pp. 102, 103.

accuracy the ability of the individual to concentrate his attention. From the testimony of those whose introspection resembles that of Külpe, the more intense the application of the attention, the plainer appears the inverse proportion in clearness between the attended and the non-attended. From the results of such work as McDougall's and Sherrington's, it appears that the more alive the one perceptual system of a sensory area the less alert is its neighbor. The concentration value, therefore, appears in the relation between the included and the excluded. In order to bring out individual differences fairly the cards containing five words and five colored letters or figures were exposed for one second in several series of experiments extending over five weeks. In the first series the subject was directed to concentrate upon the colored objects and to ignore the words. The success with which this was accomplished was astonishing. The introspection was quite uniform. After the subjects had become accustomed to the work (and not till then were any of their results recorded) the ability to select one class or the other was very marked. For all subjects I have exposed the same card directing his attention now to the words and now to the colored figures. In not one instance did the subject know he had observed the same card—so completely were the objects, of the group not attended to, inhibited. When it is remembered that there were only ten objects before the subject and that they were exposed for an entire second the phenomenon of the inhibition of half of them becomes impressively significant. There was, however, less ability to get clean-cut results when the attention was directed to the words. The reason for this is the greater difficulty in attending words (compare Tables I and II for Span). It is far easier to catch a color when the attention is upon the word-class than it is to catch a word when the attention is directed to the colors.

The table below represents the subjects by the capital letters. The first row of figures represents the number of words which entered attention when the concentration was upon the colored figures, in a series of 25 exposures of the usual cards containing five words and five colored figures.

The second row of figures represents the colors, figures and letters which entered the attention when the concentration was upon the words. Twenty exposures were made for this latter series. In both series the exposure was one second. The totals give the correlation ranking.

TABLE V

Cor. R.....	(4)	(6)	(9)	(4)	(7)	(4)	(2)	(8)	(1)	(10)
Subjects... ..	A	B	C	D	E	F	G	H	J	K
1st Series....	2	1	1	0	0	1	2	5	2	0
2nd Series...	5	8	23	7	13	6	1	10	0	28
Totals.....	7	9	24	7	13	7	3	15	2	28

I cannot believe that these totals tell us very much about the ability to concentrate and inhibit as the processes normally operate. For it would appear that 'D' is four times as apt as 'K' in concentration, whereas 'D' was suffering from a nervous depression and could hardly keep his attention on his work for fifteen minutes, when the second series was taken. While 'K' was in excellent condition, a scientist with powers of concentration trained by many years of exacting work.

The figures may not be without significance for a study of differences which relate not to general conditions of mental energy, in concentrating, but to the more minute cooperation of the 'perceptual arcs,' which need not be a series of acts protracted over a considerable period. So that an attention which could not maintain its application for any length of time could still do a very clear-cut piece of work for a few seconds. Thus the above figures tell nothing of those great types of attention which Stern described.¹

The effort to elaborate these experiments by introducing distractions was also rather abortive of results along the line of individual differences. In fact the work with distractions is rather misleading.²

¹Stern, L. W., *Ueber Psychologie der individuellen Differenzen*, Kap. viii.

²Hamlin, A. J., Attention and Distraction, *Amer. J. of Psychol.*, viii, p. 3.

With one subject the so-called distraction may act as a stimulus, with another it may be very disconcerting when he is tired and nervous, but practically ineffective when he is at his best.

In one set of tests the room was lighted by electricity and for the cards upon which the usual five words and five colored objects were displayed, I substituted brilliantly colored papers of just the same proportion as the cards to serve as backgrounds. There was sufficient strangeness in the change from daylight to electric light to make the exposure seem a little different from usual, and so to discredit any shock which the first colored background might cause. There was nothing, however, to prevent the subject from detecting the change in background as the successive exposures were made, for even in the electric light the difference between the colored backgrounds was startling, when they were placed together. The eye, of course, had to traverse the colored papers in passing from one word to another. The concentration was upon words. Table VI, below, shows the total number of words observed in the seven exposures for each subject and the number of colored squares, letters and figures which were not inhibited. The lowest row of figures shows which background was first detected by the observer. It will be noticed that the first four backgrounds, which consisted of yellow, a pea-green, a light brown and a light blue, were inhibited by all subjects.

Seven exposures were given with the usual time, one second; the usual signal of one and one-half seconds; the usual objects were presented, in fact everything was carefully arranged to give the experiments the same setting as the many series which preceded them.

TABLE VI

	A	B	C	D	E	F	G	H	J	K	L
Words.....	29	25	20	26	29	24	23	25	25	24	28
Letters and Figures.....	0	2	0	0	1	1	0	1	1	6	1
Colors only.....	0	0	0	0	1	1	0	0	1	4	1
Backgrounds (number).....	5	6	6	5	6	6	5	5	0	5	0

It would not be safe to draw any far reaching conclusions from seven experiments. It must, however, be obvious from the capacity of two of the eleven subjects to inhibit the change in background entirely that McDougall's surmise as to the inhibitory capacity of certain perceptual systems in the same sensory area is well grounded.

A comparison of the number of words attended in these seven experiments and seven similar experiments under usual conditions shows that distraction did retard many of the subjects. This a number of them felt during the experiment, but none of them assigned the influence to the right cause. Table VII shows in the upper row of figures the number of words attended to in seven experiments with no distraction, the lower row of figures shows the totals for the seven distraction experiments.

TABLE VII

		B	C	D	E	F	G	H	J	K	L
Cor. R.....	(5)	(7½)	(1)	(9)	(7½)	(2½)	(10)	(6)	(4)	(2½)	Not used
	33 29	31 25	23 26	33 26	35 29	25 24	31 23	30 25	27 25	25 24	28 32

That the distraction affected the several subjects differently is obvious. (Rank for correlations is found by subtracting the lower from the upper row.)

The auditory distraction did little better service than the visual. This is to be expected. However, they seemed preferable to such distractions as require a distribution of interest: for in such work it is hard to distinguish between distraction and an actual division of attention.¹

In the following experiments a fire-alarm bell was placed on a tin box within eighteen inches of the subject's ear. An electric attachment rang a bell the instant the shutter of

¹Darlington, T. and Talbot, E. B. Methods of Distracting the Attention. *Amer. J. of Psychol.*, ix, pp. 332-345. Also, Moyer, F. E., *op. cit.* viii, p. 405.

the tachistoscope opened, and broke the circuit the instant it closed. The noise was violent, almost intolerable to the experimenter; but in many cases it was not found objectionable to the subject engrossed in seeing the exposure. Ten experiments were made under the usual conditions of time-exposure, signal, number and character of objects exposed. The following table shows how many words were attended under the distraction conditions and how many were obtained in a series of experiments without distraction, which were chosen at random from another series. (Correlation-rank is found by subtracting the Distraction from the Non-distraction row.)

TABLE VIII

Cor. R.....	(10)	(5)	(4)	(2)	(9)	(1)	(8)	(3)	(6½)	(6½)
Subjects.....	A	B	C	D	E	F	G	H	J	K
With distraction.....	23	39	32	33	29	38	29	39	36	30
Without distraction.....	33	40	32	31	37	32	36	38	41	35

It appears that three subjects 'D', 'F' and 'H' did better with the fire-alarm than without it, and that 'C' found its presence indifferent.

If such an experiment could be varied to prevent accommodation on the part of the subjects, and could be tried a great many times on many subjects it might bring out an interesting typical trait in some subjects; namely, that the reinforcement of those sensori-motor arcs engaged in perception is conditioned by such a general agitation. Physiologically it seems to point to typical differences in the ability to adjust the great afferent currents to their appropriate motor discharges, and so brightening the vividness and the clearness of the attention.

A rather difficult experiment was made to discover, if possible, what differences of attention might appear if not perceptual matter, but conceptual matter, were material for concentration and inhibition. As Professor James says:

The immediate effects of attention are to make us perceive, conceive, distinguish, remember better than otherwise we could—both more successive things and each thing more clearly.¹

So the facility in discriminating and retaining may be considered an indication of the ability to concentrate. Upon this principle a series of fifteen experiments were made. Each card was exposed three seconds, displaying ten words. Five of these words were related to each other. Thus, some were names of parts of the body, or parts of a house, articles of furniture, kinds of animals, of colors, of fruits, etc. All words on any one card were of the same length. Nothing in the appearance of a word would indicate whether it were of the class to be attended or rejected. They were also thoroughly mixed together so that the eye had to traverse the card to observe all those of a certain class. Each subject was carefully instructed about the work and was told a few seconds before the shutter opened what class of words to seek.

The introspection was of a very similar character in all cases in which it could be given. Two subjects found that they could not recall how certain words were retained and others inhibited. It would seem that the unsought words are perceived, but scarcely recognized as of the unsought class, and instantly dropped. Probably they were not thrust from attention, but their memory is erased by the incoming correct word. This obliteration, or lack of assimilation, when one thing follows closely upon another is a familiar phenomenon in consciousness. Enough unanimity appeared in the introspection to make it evident that the eye tarried longer upon the desired classes than upon the others. This, too, would add to the process of inhibiting. In a very large number of cases the words of the unsought class would not be recognized by the subject when read to him. Indeed many cards were shown to the subjects when they had made unusually good records, and they declared that they could not have seen the words of the unsought class for not a vestige of them remained in consciousness.

¹James, W., *Psychology*, vol. i, p. 424.

The following table gives the total number of words attended to in the sought classes and the total number of those attended to in the unsought groups:

TABLE IX

Subjects.....	A	B	C	D	E	F	G	H	J	K
Words correctly attended.....	56	61	47	59	63	56	61	64	62	55
Words which should have been inhibited	29	16	14	12	16	18	14	21	11	9

If the figures for the words which were correctly attended to by each subject are used as numerators and those which should have been inhibited, as denominators, then the quotients will serve for correlation ranking:

	A	B	C	D	E	F	G	H	J	K
Cor. R.	(10)	(6)	(7)	(3)	(5)	(8)	(4)	(9)	(2)	(1)
	1.93	3.81	3.35	4.91	3.93	3.11	4.35	3.05	5.63	6.11

The above tables do not correlate with each other in a single instance. (See Correlation Tables). That is, each series of experiments tells a different story from the others. So no conclusions can be drawn concerning Concentration and Inhibition as a typical mental trait. The experiments show, only, marked individual differences in the subjects under the special conditions of each experiment.

The Mean Variations in the experiments on Span show individual differences in the constancy of attention. It varies in its efficiency so that one Mean Variation is occasionally half again as large as another. But here, again, there is absolutely no correlation between the series. The fact of variation in constancy of attention must be recognized along with the facts of variation in concentration, though they may not be shown in their relation to other mental traits.

MOBILITY OF ATTENTION

From the preceding experiments it seems clear that one class of exposures is easier for certain subjects to attend than others, from which it might be assumed that each subject would quickly choose his best class. But a series of twenty-six experiments, which were performed when the subjects had been working with the tachistoscope a few times, reveals a different situation. For in this instance the subjects were told to get all of the objects presented, if possible. They immediately tried to attend to all the words first and then the colored figures. This continued throughout the series. Subjects, who later proved that colored figures were more easily obtainable for them, in this early experiment stuck to the word list all through. The explanation is not far to seek. No matter what class of objects the attention is considering it is more natural to continue in that class than to shift to any other. The subjects were disposed to think that words were the easiest to attend to and that bias started them on the words lists. Once started upon that course they staid in it through many experiments.

Introspectively, it seems obvious that the attention stays upon one class of perceptions in preference to changing. Fechner noted this many years ago. It has often been corroborated. Every day's experience bears witness. The turning from the newspaper to composing a letter, the changing of attitude in passing from social to business affairs, the shifting of thought in passing from one picture to another in a gallery; these and a host of experiences give evidence of the 'inertia of attention.'¹

The explanation of this lack of agility in attention is to be sought in that setting of consciousness which results in adjustment of end organs, nervous system and brain paths, to receive the sensation expected. Organic adjustment, then, and ideational preparation, or perception are concerned in all attentive acts. As Wundt says:

¹Titchener, E. B., *Psychology of Feeling and Attention*, p. 246.

Every idea takes a certain time to penetrate to the focus of consciousness. And during this time we always find in ourselves the peculiar feeling of attention. The phenomena show that an adaptation of attention to the impression takes place.

Of course this has been apparent since Wundt's experiments upon reaction-time and attention; and especially since Münsterberg's more complicated reaction experiments which showed that a portion of consciousness could hold itself in readiness to do certain work at a certain time. This was also evident in those experiments in which the attention was prepared to get a certain class of words exclusive of others, (described in the last chapter.)

Such preparation of the perceptual and conceptual systems argues that certain percepts and concepts have certain courses through consciousness, that there are definite adjustments in the complexes of sensori-motor paths. So several of my subjects would say, "Now I am going to attend to this exposure with motor attention," or sometimes "with purely visual attention." They were conscious of a different setting in each case. For the colored objects the attention was largely visual, for the words it was largely motor,—in many cases the subjects repeated the words *sotto voce*. The figures and letters were sometimes attended in one way sometimes in another. The point is, however, that for the different classes of objects, different kinds of acts of attention were required. The change from one class to another demanded a change of attentional attitude. This we shall discuss more fully in a later chapter. It is sufficient to state the mere facts in the matter.

The ability of the attention to change from one to another class shows an interesting trait in the control of the attention. To bring this out clearly I arranged fifteen cards with colored geometrical forms, colored figures and letters, and monosyllabic words. These were placed on the cards in miscellaneous positions and in widely varying proportions. It was impossible to set the attention for any one class. It was necessary to change swiftly from class to class in order to obtain several objects in the one second exposure allowed.

The table shows the per cent of objects attended to the total number of objects displayed in each series.

TABLE X

Cor. R.	(2)	(3)	(1)	(8)	(10)	(7)	(5)	(6)	(9)	(4)
Subjects.....	A	B	C	D	E	F	G	H	I	K
Percentage...	56.4	53.5	59.9	42.4	40.8	44.5	52.0	48.5	42.0	52.1

So it appears that between 'C', and 'E' there is the greatest difference in mobility of attention. 'C' is nearly half again as apt as 'E' in switching the attention from one class to another.

Here, as in Concentration, *we must remember that these processes which occur in the fraction of a second may not be descriptive of all those activities which take a longer time and are more natural.*

CELERITY OF ATTENTION

That there are differences in the rapidity with which the central processes prepare to attend certain stimuli is amply demonstrated by those experiments in reaction-time where a signal is given at varying intervals before the stimulus is offered. Some individuals react better to a two-second warning, some to one second; the average preferring one and one-half seconds. So, too, we have seen that the rates of rapidity of attentional acts vary greatly in different readers. This difference was thought by Dearborn to correlate with breadth of span in attention, "The slow readers have a narrower span or working extent of attention."¹

It seems well worth comparing some experiments which bring out the subject's attitude in attending to a number of objects in quick succession and a series which indicates a span of attention.

The following experiments were performed in the spring of 1908 with different subjects from those whose results are

¹Huey, E. B., *The Psychology of Reading*, p. 178.

given in the other experiments. Let us call the six subjects 'X,' 'Y,' 'Z,' 'W,' 'A,' 'D'. 'A' and 'D' are the same subjects represented by these letters in the other experiments.

To bring out the rapidity with which the attention could pass from one subject to another, about thirty monosyllabic words, thirty black geometrical forms and thirty small colored squares were arranged in parallel columns. (A different form of tachistoscope was used from that described above. It allowed a very large field of exposure.) The subject was told to count the objects present as rapidly as possible. Three-second exposures were given. Then he was directed to write down as many objects as he could remember. The recalled objects were remarkably few in view of the fact that the subjects had seen these same words, forms and colors in other experiments many times. This phenomenon of inhibition does not interest us here, except that it shows the rapidity with which the counting was done. The remembered objects were totalled and presented in the lower row of figures in table A.

TABLE A

Cor. R.....	(1)	(6)	(3)	(5)	(2)	(4)
Subjects.....	X	Y	Z	W	A	D
Total Objects seen.....	77	49	54	51	57	52
Total objects remembered.....	11	9	8	14	11	7

In this series it appears that X is half again as quick as Y in control of his attention.

This experiment was supplemented by one in which the subjects counted eighty objects and their time of observation was carefully taken. The following table shows the time required.

TABLE B

COR. R	(1)	(4)	(3)	(5)	(2)	(6)
Subjects.....	X	Y	Z	W	A	D
Number Seconds.....	18	25.6	20	26.8	19.6	27

When these two experiments are compared they both tell the same story as we shall see later.

To get the span of attention a photograph was exposed for ten seconds and the subjects wrote down all the details they had attended to. This was done with a colored picture also. Then a series of exposures of three seconds was made for ten cards containing eight to ten colored letters and geometrical forms. The following table gives the number of objects attended to by each subject and the totals.

TABLE C

COR. R	(1)	(5)	(2)	(4)	(3)	(6)
Subjects.....	X	Y	Z	W	A	D
Picture.....	54	11	20	22	18	18
Colored Picture.....	54	12	23	19	22	18
Cards.....	87	55	66	63	76	31
Totals.....	195	78	118	104	116	67

In addition to the experiments which sought to determine the span of attention for visual perception a large number of trials was given each subject for auditory span ('umfang'). An instrument clicked uniformly for several minutes. The subject was instructed to group the clicks, without counting them, in the largest numbers possible. This was indicated by the subjects raising the hand or tapping with a pencil when a group was completed.

The following table shows the groups to represent the subject's average 'span'. The figures represent the number of clicks to a group.

TABLE D

COR. R	(2)	(6)	(1)	(4)	(3)	(4)
Subjects.....	X	Y	Z	W	A	D
Groups.....	14	4	16	8	12	8

If these four tables are correlated according to Spearman's 'Footrule' for correlating, which is explained later,¹ the following correlations appear.

¹See page 47 for explanation of the 'Footrule'.

TABLES	A	B	C	D
B.....	0.67		0.67	0.69
C.....	0.48	0.67		0.56
D.....	0.56	0.69	0.56	

The probable error is .176.

From these figures it is very evident that, under the conditions of our experimentation, there is an intimate relation between the ability to shift the attention from one object to another and the ability to grasp a large number of the objects. That this celerity and spanning of the attention are not merely a peculiarity of attention in visual perception is proved by the high correlation between the auditory 'umfang' and the results for celerity.

The results of some experiments designed to detect the relation between span and association time, performed at the Princeton Laboratory, should be noted here. The span of attention was obtained as follows. Cards with five monosyllabic words upon them were exposed for two seconds, in *Series A*, of twenty tests. While the subject was attending to the words a series of sounds (four on an average per experiment) were made by striking iron on wood, on tin, on a gong, on a steel rod, by a buzz, a series of tones on steel bars, etc. The subject sought to attend to all of the words and all of the different sounds. The scheme was reversed in *Series B*, of twenty experiments. Here the subject put his attention primarily upon the five words which the experimenter read aloud while six colors or colored forms passed before the eyes in the second during the reading. Obviously, the attention was strained to its uttermost. The total number of different sounds heard and of words seen in the first series for the two second exposures served as an index of span of attention. The second series, with the attention primarily upon the words spoken, but seeking to grasp all the colors and figures seen, supplements the first series and gives a further value in its totals for span.

Each of these series was compared with the subjects'

association time for one word and for four words. The following correlations resulted.

EXPERIMENTS COMPARED	COR. COEF.	PROB. ERROR
Span in Series <i>A</i> and Ass. Time 1 word.....	.334	.16
Span in Series <i>A</i> and Ass. Time 4 words.....	.334	.16
Span in Series <i>B</i> and Ass. Time 1 word.....	.429	.16
Span in Series <i>B</i> and Ass. Time 4 words.....	no correlation	

These results show that there is *probably* a connection between the ability to span many things in a short period and the ability to associate quickly. However, such a correlation as that which appears between the span of attention for spoken words, colored objects, etc., and association time for one word is very suggestive, when considered in the light of the results for span and quick shifting of the attention in perception. Here the connection lies between span and the quick shifting of the attention in conceptions.

SOME MEMORY FACTORS IN ATTENTION

A very noticeable difference appeared among the subjects, throughout the entire work, in the way in which the objects attended to in one experiment would lie dormant in the mind and would be reported as seen in a later exposure. By what unconscious or co-conscious process this was done no introspection could discover. In order to throw some light upon the phenomenon four sets of experiments were made. In the four sets the same cards were used. These contained 5 three-letter words and five colored letters or figures. The exposure was one second. In each set the subject was instructed to attend to the word-class in a first series, and later to attend to the color-class. In both cases he was to perceive as many objects of the class attended to as possible, but was to report *everything* he observed. In each set a number of cards were repeated, after the lapse of a certain period, to discover whether any details would appear in the second exposure which were not reported

in the first. If the influence of an exposure lingered in the memory, it should, of course, make itself evident in some difference between the repeated cards and those which were presented for the first time.

Proceeding upon this hypothesis, about twenty cards were repeated after the lapse of a week. There was nothing whatever in the results of any subject to indicate the influence of previous experiments. The results for cards never seen before were quite as large as for the repeated cards. There were no introspective results to indicate a continuance of the previous objects exposed in memory.

It was impracticable to vary the number of days between the experiments, so the next set of trials was made within an hour's experimentation. In order to repeat a sufficient number of cards in the hour a period of five minutes was allowed to elapse between the original exposure of a card and its repetition. In Table I five minutes elapsed between the first and the second exposures of the cards. In Table II there was a two minute intermission, and in Table III the card repeated followed immediately upon its first exposure. The figures in the tables are found by subtracting the results for the first exposures from those of the repeated exposures, then dividing by the number of experi-

	TABLE E		TABLE F		TABLE G		TOTALS FOR SUBJECTS	COR. RANKS
	W	L	W	L	W	L		
A.....	0.90	0.11	0.80	1.22	0.75	2.50	4.68	(6)
B.....	0.75	0.83	0.80	0.25	0.00	1.50	3.63	(10)
C.....	0.07	1.50	1.33	1.80	0.60	2.00	7.30	(2)
D.....	0.54	1.44	0.16	0.50	0.40	1.80	4.52	(7)
E.....	0.44	2.00	0.40	0.20	2.00	1.80	6.84	(3)
F.....	0.27	1.00	0.33	1.25	0.25	1.80	4.90	(5)
G.....	0.60	0.62	0.60	1.33	1.00	0.20	4.35	(8)
H.....	0.45	0.33	0.40	0.00	1.25	1.75	4.18	(9)
J.....	0.10	0.83	0.00	2.00	0.75	1.30	4.98	(4)
K.....	1.00	0.87	0.16	2.16	1.70	2.30	7.87	(1)
	0.512	0.953	0.274	1.021	0.870	1.695	Averages (for experiments)	

ments per subject. Where the subject averaged less per experiment in the repeated than in the original exposures the loss is indicated by the minus sign. ('W' = word-class, 'L' = letter-class).

The difference between the subjects is very evident from the tables; there is, however, no correlation between the subjects' averages.

In noting the greater increment in repetitions for the word-class than for the letter-class it must be remembered that the Span itself is greater for the letter-class.

The most instructive feature in the Tables is the increase in the averages for the repeated exposures. This clearly proves that something of the first perceptions of the cards lingered in memory and assisted in later observations. This influence became more marked as the time between the original and its repetition was shortened. In the repetitions which followed immediately upon the originals the cards were frequently recognized after the subject had seen two or three objects, but in the series of repetitions following two and five minutes after the originals, recognition was comparatively infrequent.

IDEATIONAL TYPES

The terms 'Memory Types,' 'Speech Types,' 'Types of Imagery', have been used very frequently to point out characteristic differences in the way in which different classes of individuals imagine, remember, and speak.

In both physiological and theoretical psychology the 'division of labor' in the mental economy is constantly emphasized. Experiments are often performed to show that memory is not a faculty dealing with all matter in the same way, but that it divided its labor among visual, motor, auditory and other functions of the mind. The work upon aphasia has been especially enlightening.

Internal speech is a revival of auditory, visual and articulatory memories, its integrity depends upon the united action of these three centers; but the one which is the most highly cultivated is revived most vividly.¹

¹Collins, J., *The Genesis and Dissolution of the Faculty of Speech*, p. 62.

Charcot and his school referred to those who were the most proficient in any one faculty as 'visuels,' 'auditifs,' and 'moteurs.' Such proficiency comes from either a natural bent or an adaptation of certain faculties to certain work. So Baldwin states:

The brain is a series of centers of relatively stable dynamic tension, the various associative connections among these centers are paths of less and more rather than least and most resistance; the range of alternative judgment is occasionally wide, and consequently any individual has his "personal equation in all functions as complex as those of speech. One man is a 'motor', a second a 'visual', a third an 'auditive', according as one or another of the extrinsic causes of stimulation suffices to release the necessary energy into his motor-speech center."¹

Not only are the activities of these complexes, that give rise to expression, conditioned by the brain paths which nature designed as the highways for her nervous currents; but, also, by those byways which much traffic has developed into highways. Could we make blue prints of these courses our charts would show some strikingly characteristic differences. And we should be thoroughly prepared to believe that the conduct of consciousness in general is obliged to adapt itself to the conditions of its thoroughfares. Certainly it would seem most probable that the "area of greatest clearness" in consciousness would correlate with the broader and more evident mental types. We find, on examining consciousness, that attention is not a fixed thing, a faculty, any more than are memory and imagination.

Yet in much of the literature of late years, in which the faculties have been scouted, I know of no author who has applied his own criticisms consistently to the attention. Memory on the other hand is now known to be a function of the content remembered, and not a faculty which takes up the content and remembers it. So we have no longer one memory, but many, visual, auditory, motor memories. Yet the very same thing is true of attention. We have not one attention but many. Attention is a function of content; and it is only as different contents

¹Baldwin, J. M., *Philos. Rev.*, July, 1893, p. 389.

attended overlap and repeat one another that they have somewhat the same function in attention.¹

The problem before us is to discover whether these ideational differences actually do affect the attention. Does a strongly visual type naturally select a different class of objects to attend to from those that appeal to the motor type? Is it easier for those perceptions which call out motor activities to be attended to by the motor type than by the visual? Do such differences stand out clearly, or are individuals now of one and now of another type? These and similar problems are now before us.

It will, perhaps, make the work appear clearer if the experiments upon types of imagery are given first. It will enable us to detect those subjects who are of a pronounced type and we may follow their results through the other experiments readily.

Galton, who with Titchener, is one of the pathfinders in mental types, prepared a table of questions whose answers indicated the subject's type.² These answers were graded according to the degree of the subject's realization of the imagery. If it was very clear, bright and distinct it headed the series; if no image at all was realized, it was graded zero. Between these extremes were seven intermediary grades. In general, I followed Galton's scheme. With this difference, however, that each subject graded himself according to the clearness and vividness of his imagery upon a scale of ten; zero being the lowest and ten the highest. The questions were read to the subjects and any misunderstandings were made clear. Ample time was allowed for deliberation. The subject considered a visual image first, then auditory, then motor. This enabled him to make frequent comparisons of the three orders and allowed for a separate effort and judgment for each question. The experiments continued through several weeks. This tended to eliminate minor differences.

¹Baldwin, J. M., *Mental Development*, p. 468.

²Galton, F., *Inquiries in Human Faculty*.

Titchener, E. B., *Experimental Psychology*, vol. i, chap. xii;

Subjects varied in their abilities from time to time. Thus 'K' who was very good in introspection found that certain kinaesthetic images would vary perceptibly. 'D' was unusually poor in his introspection; so inefficient, in fact, that all images seemed equally distinct whether visual or gustatory, auditory or olfactory. I have inserted his results for a similar series of experiments performed in the spring of 1908, when he was in better physical and mental condition. I found that the memory images of nearly all of the subjects were conditioned by the memory of the particular person or the particular thing remembered. To acquire a greater uniformity I selected purely imagery matter for consideration.

The questions were for the auditory imagery: "How clearly can you hear a harp and flute playing together; a trio, of two males and a female voice; the wind blowing through trees and sound of waves; the wind blowing a tune; the songs of the Bedouin Arabs; a conversation in Arabic; the cry of camels; the scraping of a file on a violin; the sharpening of a saw; an artillery bombardment." For the visual were asked: "Can you represent to your mind an image of a pyramid; can you see the stones clearly; the cracks between the stones; the sand wastes around the pyramid; the skies above it; can you imagine a cobweb colored red; a battlefield; a printed page with every other line in colored ink; a giraffe reaching for the leaves of a tree." For the motor imagery the following were asked: "Can you represent to yourself how it feels to write with the left hand; to wind the watch with left hand; to throw a stone with the hand not usually used in throwing; to walk backward; to lift the hat with the left hand; to walk through sand shod in sandals; to do an oriental dance; to waltz and stoop at the same time; to gesticulate like the deaf and dumb; to ride upon a camel."

When a subject was left-handed or ambi-dextrous a question was substituted for those implying a normal right-handed practice.

The following tables represent the totals of the grades for the three orders. Questions were not asked concerning gus-

tatory or olfactory images after the lists for strictly memory images were discarded

SUBJECTS	TABLE XI		TABLE XII		TABLE XIII	
	VISUAL	C. R.	(AUDITORY)	C. R.	(MOTOR)	C. R.
A.....	47	(2)	16	(10)	48	(1)
B.....	39	(5)	22	(7)	31	(6)
C.....	50	(1)	41	(2)	37	(3)
D.....	27	(9)	49	(1)	33	(5)
E.....	41	(4)	37	(3)	25	(10)
F.....	23	(10)	25	(5)	27	(8)
G.....	30	(8)	18	(8)	34	(4)
H.....	38	(6)	33	(4)	30	(7)
J.....	45	(3)	17	(9)	41	(2)
K.....	36	(7)	23	(6)	26	(9)

These tables were supplemented by a series of experiments upon associations, to discover whether the visualizer differed from the others in the class of associations or in his association-time. The subjects were also given problems, from time to time, and sought to introspect upon their habits of mind in solving the different kinds of problems. The result of this work is not given as it did not prove satisfactory. The above table, which is based upon Titchener's method gave the clearest results obtained.

VISUAL PERCEPTION AND ATTENTION

Our perceptions are directed by the heritage of brain traits and experiences. When these mark us as visualizers, our perceptions will be different from those of the motor types. In the perception of simple color there is very little motor activity. Usually there is no disposition to pronounce the name of the color or shade and the effort to recall it is distinctly visual. This is not so of words. Though they, too, are sometimes recalled usually and perceived as visual objects without an introspectively perceptible reaction, when they occur, as a usual experience they are pronounced. Frequently this is merely an internal expression; very often

it results in lip-movement, and frequently in an audible though unconscious articulation. When the distractions accompanied the effort to perceive the largest number of words possible, there was a very marked increase in motor activity which in several instances resulted in the subjects speaking aloud though they were not aware of it at the time.

In the following experiment there were exposed fifteen cards containing colored objects, figures, letters and three-lettered words. The subject was told to select as many objects as possible regardless of class. The exposures were one second. Each subject had had a long and varied experience in perceiving the classes of objects presented and instinctively sought that which was easiest. This had to be instinctive, as each card varied so in the number of the several classes of objects and the time was so short, there was no opportunity for deliberation.

In the following table the per cent of colors obtained, to total number of colors, is given:

TABLE XIV

Cor. R.	(4)	(2)	(1)	(8½)	(5)	(10)	(6)	(8½)	(7)	(3)
Subjects.	A	B	C	D	E	F	G	H	J	K
Percents.	42	54	58	30	39	26	38	30	31	51

The high figures for several subjects whom we saw classed as visualizers in the ideational type experiments is practically explainable by the fact that they have a very wide span of attention for the color classes, as may be seen by comparing their results for 'span,' and they would get large results in such cases. We shall speak of this later.

Another experiment involving color, and used in another connection, will assist in the present problem. Twenty-six one-second exposures were made for cards containing five words and five colored figures. The attention was upon the colored figures primarily and the subject was to perceive both figure and color and associate them together in his report. This involves motor as well as visual factors. The total num-

bers of associated colors and figures are given for each subject in the table below:

TABLE XV

Cor. R.....	(1)	(6)	(3)	(2)	(7)	(8)	(10)	(5)		(9)
Subjects.....	A	B	C	D	E	F	G	H	J	K
Totals.....	67	28	38	39	22	21	6	30	35	8

The totals of all the colors perceived out of a possible hundred and thirty for this same series are given in Table XVI

TABLE XVI

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(1)	(7)	(2)	(10)	(3)	(8)	(9)	(5½)	(5½)	(4)
Total Colors.	92	53	73	35	69	42	37	54	54	67

Another experiment was made in which five colors and five geometrical forms were presented in one second exposures. In all there were fourteen exposures with the attention directed to the colors which were simple and easily perceived and recalled. The totals for the colors attended are as follows:

TABLE XVII

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(5)	(7)	(3½)	(9)	(6)	(8)	(10)	(2)	(1)	(3½)
Totals.....	62	54	63	42	55	43	27	64	66	63

One of the most interesting of these experiments was performed as follows: ten brightly colored squares were exposed for one second, the subject was to perceive as many as possible and report. After fifty such exposures were made five words were substituted for five of the colored squares. The change in the character of the cards was not announced until subjects discovered it themselves. All through the series the the instructions were to get all that it is possible to perceive

no matter what it is. The result was that some subjects continued to get colors and some switched over to the words. There was probably no fatigue, certainly not enough to count for any changes in the direction of attention. The choice of the word class after the momentum of fifty experiments in the color class is indicative of a natural preference for the word class.

In Table XVIII the totals for the colors in the first fifty experiments are given. And in Table XIX the per cent of colors in relation to total perceptions are given. (The former is Table I for Span.)

TABLE XVIII

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(4)	(7)	(1½)	(9)	(3)	(10)	(5½)	(8)	(5½)	(1½)
Total Colors..	215	205	223	161	222	141	213	199	213	223

TABLE XIX

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(1)	(6)	(4)	(7)	(5)	(9)	(10)	(3)	(8)	(2)
Percent of Colors.....	100	57	64	44	60	30	21	69	41	85

In the Table of Correlations a number of significant correlations appear between these experiments on the visual factors in attention. Thus, this last table correlates with tables XV, XVI and XVII. The meaning of this will be discussed in the Conclusion; here it is sufficient to call attention to the fact that this class of experiments do not correlate with a single one of the experiments in the next chapter designed to bring out motor factors in attention.

MOTOR FACTORS IN PERCEPTUAL ATTENTION

The common experience was that the words called forth the 'inner speech.' Each one was pronounced. At times the subject would pronounce the word 'Bug' as 'Rug,' and when

giving his report would repeat it as he pronounced it to himself, but it would seem wrong and after a little thought he would correct it by visual memory. This was not at all common. The motor-auditory process usually predominated, especially in memory. For it was a universal experience that the words meant nothing. They were as so many nonsense syllables until reported, and within two minutes after the report, they were forgotten. Without the co-operation of the 'inner expression,' the mere visual perception of the words would have been retained very poorly. From the experiments in reading it would seem that this inner speech is an incipient movement. It does not effect the larger or chest muscles perceptibly, nor does it produce lip-movements necessarily. It is a motor activity which apparently varies as the effort to make the perception clear and strong varies. It is marked in children learning to read. I have found it very pronounced when learning a strange language, but it is diminished as the language was acquired. Quantz thought:

It is a specific manifestation of the general psycho-physical law of dynamogenesis by which every mental state tends to express itself in muscular movement.¹

Our interest is to discover whether those who appear, according to their own introspection, to be clearly motor types are influenced in the direction their attention instinctively takes, in the effort to grasp as many objects as their span will allow, when these objects call out more or less of the motor activity.

To bring this out a series of fifteen exposures was given the subjects, of cards containing five words and five colored letters, numbers or figures. The subjects were told to make their own selection of class of objects, but that they must attend to the largest number possible in every case. In Table XX the per cent of words to the total number of objects attended to is given.

¹Quantz, J. O., *Psychol. Rev.*, Mon. Supp., vol. ii, No. I. See also, *Philos. Rev.*, ii., pp. 385-407; *Psychol. Rev.*, 1894, pp. 441-453; *Yale Studies, Psychol. Laboratory*, ii. p. 122.

TABLE XX

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.	(4)	(1½)	(8)	(7)	(9)	(3)	(1½)	(5)	(10)	(6)
Percent of Words.....	73	81	60	62	38	74	81	72	37	65

The differences in the results for the subjects are sufficiently large to show individual preferences which may be taken as indicative of mental traits as affected by this kind of experiment. What these traits are does not appear, for this series does not correlate with any other in the whole Table of Correlations.

After the lapse of several weeks another series of exposures was made in which the subjects were again directed to seek that class which is the easier to attend to. In this case there were five words and five colored figures. Twenty one-second exposures were made. The totals for word are given in Table XXI.

TABLE XXI

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.	(8)	(10)	(5½)	(5½)	(7)	(3)	(4)	(9)	(2)	(1)
Total Words.	60	42	72	72	69	77	76	48	85	86

This table not only does not correlate with the preceding experiment of like character; but it also fails to correlate with any of the others except those represented by Table IX in Concentration and Inhibition. From this correlation it would appear that the disposition to turn the attention to words in preference to colored objects and the ability to set the attention for a certain class of words and to inhibit others, are faculties which go together.

In the next experiment the subjects concentrated upon the words only. They sought to inhibit all else. The purpose in this was to discover whether there were any correlations between the ability to attend to the word class and the other

acts of attention involving motor factors. There were six three-letter words and six colored figures on each card. Twenty exposures of one second each were made. The total number of words attended to are given for several subjects in Table XXII.

TABLE XXII

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(8)	(2)	(9)	(7)	(3)	(10)	(5)	(4)	(1)	(6)
Totals.....	66	79	64	70	78	63	75	76	82	71

Here, the only correlation is with the experiments for auditory factors. In this case the high correlational coefficient of .51 indicates a relation between the ability to concentrate upon the word-class and the ability to attend to the reading of poetry while viewing the presentation of a succession of words. (See Table XXVIII in chapter on Auditory Factors.)

Table XVIII of the last chapter may be used to find the per cents of those who, having observed colors for fifty experiments detected the words and attended to them when they were added to the color-cards. The per cent of words to total number of objects seen,—when the words were added, is given in Table XXIII; which is Table XVIII of the last chapter reversed.

TABLE XXIII

Subjects.....	A	B	C	D	E	F	G	H	J	K
Cor. R.....	(10)	(5)	(7)	(4)	(6)	(2)	(1)	(8)	(3)	(9)
Percent of Words.....	0	43	36	56	40	70	79	31	59	15

This series makes two interesting and important correlations with the experiments on Concentration. The first correlation is with the experiments which were designed to show the subjects' ability to concentrate upon words or colored objects to the exclusion of other classes of objects. The correlation here points to a relation between an aptitude for the word-class and the ability to expend the attention upon one

class exclusively. This hints at what has already been noted, that the greater activity of attention required by the word-class demands a greater effort; and, further, appeals to those to whom this kind of effort is natural and congenial. The second correlation is with the series of experiments in which the subject set his attention for a certain class of words before the exposure, and inhibited all of the other classes. Here the faculty of turning instinctively to the word-class and the ability to set the attention to perceive a certain class of objects appear related. This may mean that the motor factors which make it easier for certain subjects to attend to the word-class also make it easier for them to react to a word for which they are seeking. The efficiency then, in both cases would be attributable to a characteristic readiness of motor response.

AUDITORY FACTORS IN PERCEPTUAL ATTENTION

In the experiments upon aphasia it has been shown that the motor-auditory complexes play as important a role, in the processes of attention which accompany speaking and reading, as any other. The inner speech, of which we spoke in the last chapter is, according to Huey, a combination of motor and auditory elements, with one or the other predominating according to the subject's habitual mode of imagining.¹ Huey and Dodge both agree that the motor element is present with those who auditize in reading and that the auditory element is present with those who motorize. Huey believes that these factors may not be so prominently present with visualizers.

Now, our interest is to learn what differences characterize this motor-auditory type in their attentional processes. They should find those perceptions which can be the more readily dealt with by the habits and traits of their motor-auditive systems easier to bring into the 'area of greatest clearness' than perceptions which do not call out such reactions. We should be justified, in the light of experiments previously described, in performing a series of experiments in which the

¹Huey, E. B., *The Psychology and Pedagogy of Reading*, p. 120.

competition between the visual-motor and the auditory-motor would indicate which was the more characteristic of the subject's type.

Such a series of experiments was undertaken as follows: the subject sat before a rotary tachistoscope in which several words were exposed clearly to view during each exposure. While the subjects saw and read these words two lines of a poem were read to them. The attention in the first series of experiments was directed to the words, but the subject was also told to retain as much of the poetry as possible. When the report was given, the words were recited first, then the poetry. The purpose in having the attention thus divided, rather than free to choose either class of perceptions, was this; it is impossible to present auditory and visual stimuli with such equality that the one does not obtain some advantage over the other. It is better therefore, to give an advantage first to one and then to the other by distributing the attention consciously, and placing its emphasis on one or the other. This would give the advantage first to one type and then to the other. This is what was done. After a series of twenty-three experiments (Tables XXVII and XXVIII) in which the attention was primarily upon the visual impressions, another series (Tables XXV and XXVI) of twenty-two experiments was made with the attention directed to the auditory impressions. The per cent of words to the total presented is given in the left-hand columns of both tables. The accuracy with which the poetry was attended to was graded with one hundred as the highest count. The results for poetry appear in the right-hand columns. Care was taken that the poetry should be simple and the lines of sufficient brevity to enable each subject to grasp it no matter how inefficient he might be in memorizing lines read aloud.

In Table XXV are given the words perceived when the attention was on poetry, and in Table XXVI the grades for the amount of poetry attended in the same series are given. In Table XXVII appear the word totals when the attention was directed primarily to words, and in Table XXVIII are the grades for the poetry in this series. Table XXIV gives

the sums of the grades for the poetry, and the totals for the words together, for each subject. These indicate the span of attention for visual and auditory perceptions combined.

TABLES.....	XXV		XXVI		XXVII		XXVIII		XXIV	
	WORDS	C.R.	POETRY	C.R.	WORDS	C.R.	POETRY	C.R.	TOTALS	C.R.
A.....	78.8	(2)	93.4	(4)	96.7	(1)	57.9	(5)	326.8	(2)
B.....	82.3	(1)	81.3	(6)	93.3	(2)	80.8	(1)	337.7	(1)
C.....	45.8	(7)	80.6	(8)	76.4	(8)	32.6	(7)	235.4	(7½)
D.....	65.3	(3)	48.0	(9)	71.5	(9)	31.1	(8)	215.9	(9)
E.....	56.6	(4)	92.6	(5)	76.8	(7)	65.6	(3)	291.6	(4)
F.....	34.2	(9½)	94.4	(2½)	82.4	(5)	24.4	(10)	235.4	(7½)
G.....	44.4	(8)	43.2	(10)	85.7	(4)	26.8	(9)	200.1	(10)
H.....	54.1	(5)	95.4	(1)	80.5	(6)	50.0	(6)	280.0	(5)
J.....	47.7	(6)	81.0	(7)	89.2	(3)	76.7	(2)	294.6	(3)
K.....	34.2	(9½)	94.4	(2½)	71.4	(10)	58.0	(4)	258.0	(6)

The most obvious result of these tests is that there are very wide differences in the abilities of the several subjects to attend to what is being heard and what is being seen at the same time. The order of ability to 'span' both visual and auditory presentations clearly corresponds with the order found for span when the perceptions were visual and visual-motor. This appears in several correlations between the Tables for Span and those above, (see Table of Significant Correlations). This correlating of the visual, motor and auditory factors in attention confirms what was said in the chapter on Celerity, that the experiments measure actual differences in the attentional processes, and not mere eccentricities of perception.

The primary question in this chapter is; do those who have the clearer auditory imagination give any evidence of this trait when the attention is directed to auditory perceptions. The answer is in the high correlation coefficient for Table XXIV above, and Table XII, Ideational Types, (Auditory Type.) This correlation is the highest the Auditory Type yields. Indeed there is but one other correlation with auditory type in the entire work. There is, however, no

correlation between Auditory Type and any of the other Tables for Auditory Factors.

The Visual Type has as high a correlation with the Table XXIII, above, as does the Auditory. But it must be remembered that the visualizer has a broad span, as numerous correlations throughout have shown, and it is to be expected that his results in these experiments would total high. The Visual Type, also, shows a correlation with the ability to perceive words when the attention is engaged in hearing words. Both of these aptitudes may be due to the facility with which the visualizer catches the objects of visual perception leaving other energies of the attention free to engage with other things. This the Motor Type could not do, for his sole correlation with these tables is in the ability to attend the word series when the attention was upon the words, primarily; which bears out what has been observed before, that when the words are attended with a motor reaction, as they usually are by the Motor Type, there is little attentional energy left to be occupied with aught else.

PART III. CORRELATIONS

In the present chapter we shall compare the entire list of experiments to discover what traits of attention are related. The most satisfactory way to accomplish this is by presenting the results in one great correlation table. The significant correlations in this table are presented in a smaller table in order that they may be more readily seen and studied. (See tables at end of monograph.) After considering a number of methods for the comparing the results of the experiments, Spearman's 'Footrule' for measuring correlations was adopted. The method is explained in full in the article entitled *Footrule for Measuring Correlation*, by C. Spearman, in the *British Journal of Psychology*, vol. ii, pp. 89-108. Briefly, the method is this: the subjects are arranged in the order of their ability for two sets of experiments; for example, the two experiments recorded with their results in Table XXI in the chapter on Motor Factors and Table IX in the chapter

on Concentration. The second series of results is compared with the first and the sum of the gains in rank for the several subjects carefully noted. Thus:

SUBJECTS	MOTOR FACTORS	CONCENTRATION	SUM OF GAINS
	Table XXI	Table IX	
A.....	8	10	2
B.....	10	6	
C.....	5½	7	1½
D.....	5½	3	
E.....	7	5	5
F.....	3	8	
G.....	4	4	
H.....	9	9	
J.....	2	2	
K.....	1	1	
Total.....			8½

The sum of gains in rank is denoted by Σg in the following formula.

Let the Σg to be expected on an average, for mere chance be denoted by M ; this amounts to $\frac{n^2 - 1}{6}$ when n is the number of cases in each series. (For proof see page 105 of Spearman's article). Then the coefficient, say $R = 1 - \frac{\Sigma g}{M}$.

In the present experiments $n = 10$. So that $M = \frac{10^2 - 1}{6} = \frac{99}{6} = 16.5$. Then the correlation for the above tables will be

$$R = 1 - \frac{\Sigma g}{M} = 1 - \frac{8.5}{16.5} = 0.4849.$$

It must be remembered that in this (and in almost every) probability formula, any experimental result such as R has no scientific significance—except negatively—unless it be at least twice as great as its probable error; for otherwise it is almost as likely as not to be a chance coincidence. To be fairly good evidence, the R must be over three times greater than its probable error (*Ibid.*, p. 96).

In the following tables the decimal is carried only to the second place as the numbers are sufficiently far apart to make decimals of the second and third places unnecessary.

The Probable Error may be taken with sufficient nearness as being $\frac{0.43}{\sqrt{n}}$. (For proof see p. 106, *Ibid.*) The Probable Error in the following Table is:

$$\frac{0.43}{\sqrt{10}} = \frac{0.43}{3.162} = 0.136.$$

In this way we learn that our correlation in the above tables is just about large enough to be beyond reasonable suspicion of chance coincidence.

Positive correlations point out relationships which actually exist between two mental traits. The negative correlations show that the traits compared do *not* exist in conjunction, but that where we find one the other will be absent. Negative correlations are of value in corroborating, or contradicting the positive.

In the correlation formula used a large negative correlation may be changed to a positive if one of the two series being compared is inverted. Thus, if the sum of the gains in the second of the two series totals 11.5 the correlational coefficient will be 0.30. If the order of this second series were reversed the sum of the gains will be 19.55 and its *R* is .18.

The table of significant correlations contains all those positive correlations which are above twice the probable error. In the groups discussed below only those correlations which are above three times this probable error are considered; for, as Spearman points out, a low correlation is not trustworthy. It is important, however, if it occurs frequently. For that reason the smaller figures appear in the positive correlation table. In arranging the correlation results, below, each experiment is given and its correlations with the others. The correlational coefficient is given in parentheses and the experiment is briefly described with an abbreviated reference to the chapter and table where it is described in detail.

*Span***Totals for all experiments in Span (Span IV):**

- Correlate (o. 54) with Visual Imagination (Ideat. Types XI).
- Correlate (o. 57) with Ability in Attending color-class (Vis. Per. XVI).
- Correlate (o. 42) with Adherence to color-class (Vis. Per. XIX).

Span of Attention for Colored Objects (Span I):

- Correlate (o. 63) with Instinctive Selection of color-class (Vis. Per. XIV).
- Correlate (o. 54) with Ability in Attending color-class (Vis. Per. XVI).
- Correlate (o. 45) with Retention in Fringe of Attention of former objects seen (Mem. Factors).
- Correlate (o. 45) with Visual Imagination (Ideat. Types XI).

Span of attention for Colored Letters And Numbers (Span III):

- Correlate (o. 66) with Ability in Attending color-class (Vis. Per. XVI).
- Correlates (o. 45) with Visual Imagination (Ideat. Types XI).

Correlations between the several Experiments upon Span.

- Span for colored objects (Span I) correlates with span for colored letters and figures, (Span III) and the Totals for all Span Experiments (Span IV), Correlate (63) with Span I, (42) with Span II and (o. 78) with Span III;

*Concentration and Inhibition***Ability to concentrate upon one class of Objects (Concent., etc. V).**

- Correlate (o. 51) Preference for word-class (Motor Fac. XXIII).

Ability to concentrate when objects are upon Colored Backgrounds (Concent. VII).

- Correlate (o. 45) Ability in color-class (Vis. Per. XVII).

Ability to concentrate upon words of a certain Class (Concent. etc. IX).

- Correlate (o. 48) Instinctive Selection of the word-class (Motor Fac. XXI).

*Ideational Types***Visual Imagination (Ideat. Types XI):**

- Correlate (o. 57) with Ability in Attending color-class (Vis. Per. XVI).
- Correlate (o. 51) with Ability to Associate colors with their objects (Vis. Per. XV).

Correlate (0.42) with Instinctive Selection of the color-class (Vis. Per. XIV).

Correlate (0.48) with Span of Auditory Attention (Aud. Factors XXIV).

Auditory Imagination (Ideat. Types XII).

Correlate (0.48) with Span of Auditory Attention (Aud. Factors XXIV).

Correlate (0.42) Concentration of Attention during Fire-alarm distraction (Concent. etc. VIII).

Motor Imagination (Ideat. Types III).

Correlate (0.45) with Ability to associate colors with their objects (Vis. Per. XV).

Motor Factors, etc.

Ability in Attending the word-class (Motor Fac. XXII).

Correlate (0.51) with ability to attend Poetry read while concentrating primarily on words seen (Aud. XXVIII).

Ability in attending the word-class (Motor Fac. XXI).

Correlate (0.42) with memory experiments.

Visual Perception, etc.

Instinctive selection of color-class (Vis. Per. XIV).

Correlate (0.45) with the ability to shift the attention from one class to another (Mob. X).

Ability in Attending the color-class (Vis. Per. XVI) correlates.

(0.42) Totals for Auditory work (Aud. XXIV).

The Adherence to the color-class when the word is introduced (Vis. Per. XIX).

Correlate (0.42) with Auditory spanning of attention. (Aud. XXVI)

The Experiments upon Visual Perception and Attention correlate among themselves as follows. The ability to attend the color-class correlates (0.48) with the adherence to the color-class when the word-class is also introduced. The two similar experiments upon ability to attend the color-class correlate (0.42) with each other.

Auditory Factors in Perceptual Attention

The majority of correlations between the auditory experiments and the other experiments appear in the above groups. It remains for this group to note simply the correlations of these experiments with each other. The totals for Span in all the auditory work (Aud. XXIV) correlate (0.60) with the ability to attend to poetry read when the attention is primarily upon words seen (Aud. XXVIII), and the totals, also correlate (0.48) with the ability to attend the words seen in the same tests (Aud. XXV). It is also noteworthy that the totals for these experiments give a ranking which correlates with three out of the four of the constituent experiments.

From the above correlations it may be inferred that the observer who is broad-spanned for all classes of objects seen is of the visual type of imagination. Further, he shows a preference and a unique ability in attending to the color-class of objects. The ability to attend to a large number of colored objects is coupled with an instinctive preference for that class and is related to the faculty of carrying the impression of former observations in the fringe of attention.

The results for concentration are rather scattering. The knack of inhibiting the distraction of a colored background is related to special ability in attending to the color-class. Why this should be so is hard to understand. The ability to concentrate upon the word-class and to 'set the attention' for a certain class of words both seem to be connected with a fondness for the word-class. This may be due to a motor setting of the attention.

The Ideational Types are most instructive in the relation the Visual Imagination sustains to visual perception. There can be little doubt that the visualizer is more successful in attending to the color class than to the words. It is curious that the visualizer also does well in the auditory work. The very few correlations that the Auditory Type of Imagination makes renders its correlation with span for auditory work significant. This correlational value with auditory span is the same for the visualizer, and it must be remembered that the attention

was directed to visual as well as auditory perceptions in the auditory work. An unusual correlation is that between the ability to span visual and auditory perceptions synchronously and the ability to inhibit auditory distractions. The connection between the motor type of imagery and the ability to connect a color with the object upon which the color appeared would seem to imply a relation between visual and motor retention of objects observed.

The connection between the faculty for attending to the word-class and for attending to poetry while viewing words may point to a motor repetition of the verses which would be easier for the motor than the visualizer. Ability in the word-class correlates with the holding of objects previously observed in the fringe of attention.

In the experiments upon Visual Perception and Attention the most significant fact is the correlation between the different experiments in this same class. It is evident that there is some underlying trait of attention which appears in these tests. It is probably an ability to attend, remember and imagine in visual terms better than in others.

The auditory-visual experiments show rather scattering correlations. Their correlations among themselves show they point in one general direction. The lower correlations show an interesting relation to span for visual work.

A comparison of the Tables of Significant Correlations and Negative Correlations is one of the best indications, that the experiments actually point out typical traits in Attention and that the results are not accidental.

If the Significant Correlations were in about the same proportion as the Negative for each set of experiments, it would be apparent that the whole work is unreliable, and the results fortuitous. This is not the case. Following the correlations for Span across the Table it appears that the Positive and Negative correlations, which are significant, occur in the following proportions; 3 : 0; 2 : 6; 0 : 0; 3 : 0; 10 : 1; 0 : 7; 5 : 1; 2 : 0. Those for Visual Perception are much more striking; 8 : 0; 0 : 13; 7 : 0; 1 : 1. The negative correlations support the positive in nearly every case. The results for

Concentration and Inhibition are, as might be expected, the least reliable.

The negative correlation between concentration experiments and Span is a further support to what has been found before, that span and concentration do not seem to be related.

The many negative correlations between Span and Motor Factors show conclusively that the motor element in attending to the word-class is a feature of attention which does not go with Span.

It is interesting to note that the only positive correlation Mobility makes is with Instinctive preference for color-class and the only high negative is with Preference for the word-class.

The five negative correlations between Visual and Auditory Types and the three experiments on Motor Factors are not high enough to carry much weight but they are suggestive in showing a negative relation between Motor Factors and the Visual and the Auditory Types but not the Motor Type.

The many negative correlations between the experiments with Motor Factors and in Visual Perception show again, that the series of experiments affected attention in characteristically different ways.

In summing up the results of the Negative Correlations it may be said that they corroborate the findings of the Positive Correlations, but do not add to our information materially. A casual comparison of the two Tables shows this clearly. A more exact study brings to light the fact, noted earlier in the text, that those experiments whose results were not entirely clear (such as the work in Concentration) have a more even proportion of positive and negative correlations than do the experiments upon Span, Types, Motor and Visual Factors, etc. In the latter the proportions for the two classes of correlation are thoroughly consistent and convincing.

CONCLUSION

From the foregoing correlations and discussions the following conclusions are deduced, concerning Types of Attention,

under the conditions of experimentation described in the preceding chapters.

1. There are broad and narrow spanned types of attentional activity. The broad spanned type for visual perceptions is also broad spanned for auditory perceptions (umfang).

2. There is also a type of attention which is alert, active, under quick control; and there is a type which moves sluggishly. The former is broad spanned.

3. The ability to concentrate and inhibit does not appear in close relation with any other marked traits of attention. This ability varies in individuals but not in a manner which gives evidence of type.

4. The dexterity or suppleness in control of attention is another feature which cannot be classed as a Type.

5. The impressions which catch in the 'Fringe' of attention and later enter the 'Clearness Area,' vary characteristically with individuals. The type most susceptible to this experience is also broad spanned.

6. The Visualizer is broad spanned for both visual and auditory perceptions.

7. The 'Auditif' shows his attentional type in the ability to inhibit sound and in the breadth of span for visual and auditory impressions presented synchronously.

8. The Motor Type of ideation makes so few correlations that its evidence is largely negative. It is not broad spanned for the work given in these experiments. It is probably more efficient in concentration than the Visual.

In view of these results it must be acknowledged that the Attention is a function of the co-operation of many factors of the mind and it takes its character from them. The activities of the Attention will not be understood until the relation to these component and controlling factors is understood. As Goethe has well said:

"Das Besondere unterliegt ewig dem Allgemeinen
Das Allgemeine hat ewig sich dem Besonderen zu fügen."

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On the Functions of the Cerebrum: the Occipital Lobes.

Shepherd Ivory Franz, Ph.D.,

Research Assistant of the Carnegie Institution of Washington;
Scientific Director and Psychologist, Government Hospital for the Insane;
Professor of Physiology, George Washington University.

with the cooperation of

Gonzalo R. Lafora, M.D.,
Histopathologist, Government Hospital for the Insane.

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INTRODUCTION¹

The positive statements regarding the functions of the occipital lobes might lead one unacquainted with the facts to believe that the functions of these portions of the cerebrum were well understood. A careful consideration of the experimental and clinical phenomena accompanying disturbances in these areas does not support the general belief, and although they are among the first parts of the brain to have been investigated by physiological methods there remains much to be done before we may say we understand their function and their method of working. We may apply directly to this region of the cerebrum the statement of Hitzig, made a decade ago in regard to the whole brain: "Die Hirnrinde und ihre Zusammenwirkung mit ihre subcortikalen Centren stellen vielmehr noch heute einen dunklen Continent vor (31)." That to some extent and in some manner the cortex in the occipital portion of the brain functions in connection with the retinae is probably true, but how it functions and in what manner it is connected with the retinae have not been determined

¹The present work was made possible by a grant from the Carnegie Institution of Washington to the author as research assistant. This assistance is hereby acknowledged and the author expresses his sense of obligation for it. On the histological side of the work the author has had the opportunity of the coöperation of Drs. N. Achúcarro and Gonzalo R. Lafora, respectively former and present histopathologist at this institution. Assistance in the observation and in the training of the monkeys has been given especially by Dr. A. H. Sutherland, formerly of this institution and now of the University of Illinois, and by Drs. Achúcarro, Barnes, Blackburn, and Lafora and their assistance is hereby gratefully acknowledged.

The present work deals mainly with the lateral portions of the occipital lobes, and it is expected that articles will appear later dealing with the mesial aspect and with the results of electrical stimulation.

While this article was in manuscript form there appeared von Bechterew's *Ueber die Funktionen der Nervencentra*, which contains a resumé of much work on the occipital lobes. Von B. discusses to great length the work of Goltz, of Munk and of Luciani but slights much which is discussed in pp. 9-23.

to the complete satisfaction of those who have most carefully investigated the matter.

The numerous recent histological studies of the architecture of the cerebral cortex emphasizes the fact that the clinical and physiological problems of the localization of function are not simple, either in method or in interpretation. Although there is considerable diversity in the results of the different histological investigators, there is, on the other hand, sufficient uniformity to indicate that the old landmarks of the cerebrum must be given up and that the facts must be viewed in connection with the plans made by recent surveys. Most of the fissures, especially those of a secondary character, are of little value for the limitation of special areas, and it is necessary at the present time to consider the areas from an entirely different standpoint. This standpoint is the intimate structure of the cortex, the number and grouping of the cells, the arrangements of the fibers, and the combination of these. It has been shown that we may almost entirely disregard the cerebral fissures and divide the cortex of the cerebrum into approximately fifteen areas, each with an histological structure quite distinct and different from those of the surrounding areas.

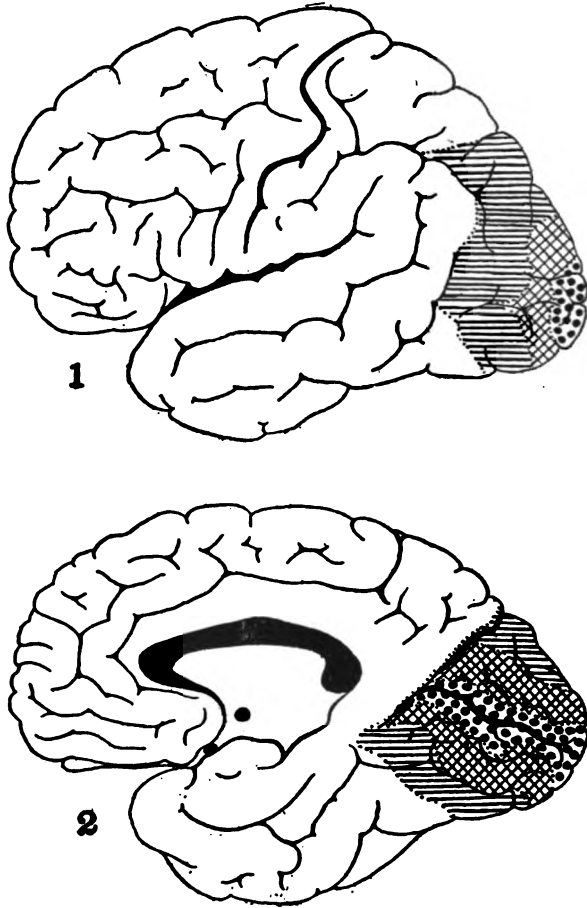
The histological studies have led to the formulation of hypotheses regarding the probable function of the parts and the statement is made that each of the areas histologically distinct is also physiologically different from the bordering areas. Although all the histologists appear to give to their results an interpretation more or less physiological, Brodmann (9, 10) in particular has expressed this stand more plainly and more positively than most of the investigators. He has written: "Es scheint doch näher zu liegen und vom histologischen Standpunkte aus geradezu geboten, spezifisch differenten Rindenzellen auch qualitativ differente Funktionen zuzuschreiben;" and in a further discussion of the matter: "Die spezifische histologische Differenzierung von Rindenarealen beweist unwiderlegbar deren spezifische funktionelle Differenzierung" (9). This leads directly to the anatomical division of cerebral function, to a sort of phrenology, and to an anatomy of mind which will not bear criticism.

THE OCCIPITAL LOBE

The gross appearance and the extent of the occipital lobe may be learned from the text books of anatomy (61), or will be recalled by a glance at figs. 1 and 2. The lobe comprises about one-eighth of the superficial part of the cerebral cortex. On the mesial aspect of the hemisphere the calcarine fissure is included in this area, and its upper and anterior limits are roughly fixed by the parieto-occipital sulcus. On the lateral aspect it reaches to the transverse occipital fissure. In the fresh as well as in stained sections of part of this region there can be seen in the cortex without the aid of the microscope a distinctly lighter streak, apparently dividing the cortex into two layers. This is the line of Gennari which, both macroscopically and microscopically, gives to this area an appearance very different from all other parts of the cerebral cortex.

Further examinations of the cortex by histological methods reveal other differences. The larger region is found to have two or three different types of arrangements of cells and fibers, so that by the more careful methods it is possible to distinguish and to differentiate two or three areas of anatomically (histologically) distinct types. The most characteristic of these areas is the one in which the line of Gennari is prominent, an area which may be called the calcarine area. This area surrounds the calcarine fissure, and in man reaches beyond the occipital pole toward the lateral aspect of the cerebrum for only a very short distance. The limits of this area have been accurately determined by a number of investigators, all of whom report essentially the same results. The structure of this region is described by Campbell as follows (11, p. 17): "The characters which distinguish the calcarine type of cell lamination are, first, the almost unique layer of large stellate cells usurping the position occupied by the external layer of large pyramidal cells in other regions; secondly, the existence of pale-stained zones above and below the uncommonly well-marked layer of stellate cells, the upper of which marks the position of the line of Gennari; thirdly, the presence in the depths of the cortex of the layer of solitary cells of Meynert, cells which differ from homonymous cells in any other part of

the brain." The remainder of the occipital lobe is considered by Campbell to be one area, while Brodmann has been able to distinguish two separate areas, which he admits are closely related (10, p. 228). The cortex surrounding the calcarine area,



FIGS. 1 and 2. The human cerebrum, with the areal differentiation of the occipital lobe. The dotted part is the calcarine type, and the parts marked by horizontal lines and by cross hatching are the remainder of the so-called visual cortex. Adapted from Brodmann, and reduced.

and extending 1.5 to 2 cm. beyond it, is supposed to have a function somewhat similar to that of the calcarine area, in that it is intimately related to the calcarine area, and has been correlated

with certain clinical findings relating to vision. To this region Campbell gives the name visuo-psychic as distinguished from the calcarine which is called visuo-sensory.² The structural appearance of this surrounding area is somewhat similar to that of the calcarine area, but sufficiently different from the latter to be considered distinct. The cortex is of greater depth, the plexiform and small and medium-sized pyramidal cells are all appreciably deeper, but the latter two are less numerous; the large pyramidal cells are mixed with the other cells and do not form a layer by themselves; the layer of stellate cells has an appearance similar to that in the calcarine area, and the other cell elements are not divided into layers as in the calcarine region.

Figs. 1 and 2 illustrate the division by histological methods of the occipital cortex in man by Brodmann. Here is shown the



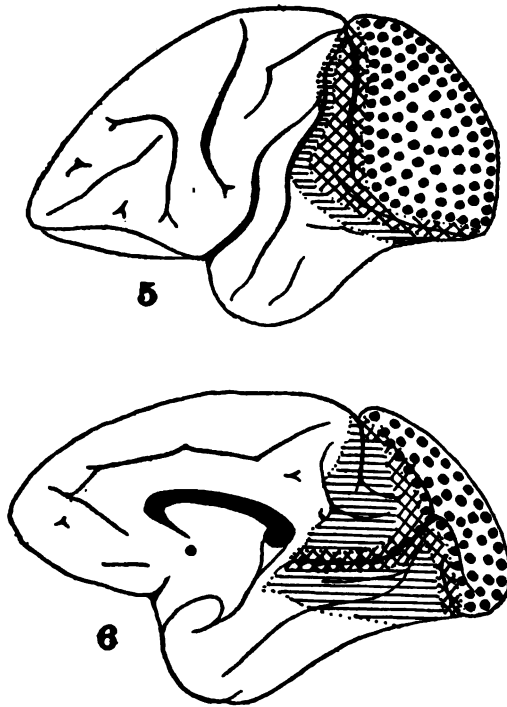
FIGS. 3 and 4. The occipital pole of the human cerebrum, with the areal distributions of the so-called visuo-sensory and the visuo-psychic regions. The dotted portion is the calcarine type of cortex, and the cross hatched portion is the visuo-psychic area. Adapted from Campbell. 3, the mesial aspect; 4, the lateral aspect.

calcarine area, marked by large dots, surrounding that fissure. and in the neighboring regions the two areas closely related to the calcarine but differing from the latter in cell and fiber structure. The two areas, as has been indicated above, are not absolutely distinct and have been grouped by Brodmann to make one area. By this grouping Brodmann closely approaches the divisions made by Campbell (11), by Elliot Smith (69) and by Bolton (8), as well as those made by the earlier investigators.

²These names were previously used by Bolton.

The division of the cortex by Campbell is given in figs. 3 and 4, which should be compared with the division by Brodmann in figs. 1 and 2. It will be seen that the total extent of the cortex supposedly devoted to visual function is approximately the same in the two figures, the apparent difference being accounted for by the different views of the brain which the two investigators have used for the delineation of the areas. The most marked difference between the areal distributions of Campbell and Brodmann is the larger relative size of the calcarine cortex outlined by Campbell. The surrounding region is, however, not so easily differentiated from the remainder of the brain as is the calcarine cortex, and the variations of the individual observers may be due to actual differences in material, as Bolton (7) suggests in regard to another matter. Bolton reports that in an examination of brains for the exact localization of the visual cortex he found individual differences apart from the age and the visual abilities of the people whose brains he examined, and he remarks that "as such differences exist in a projection area, it is possible that more marked variations will occur in the case of the later specialized areas of different brains" (7, p. 309).

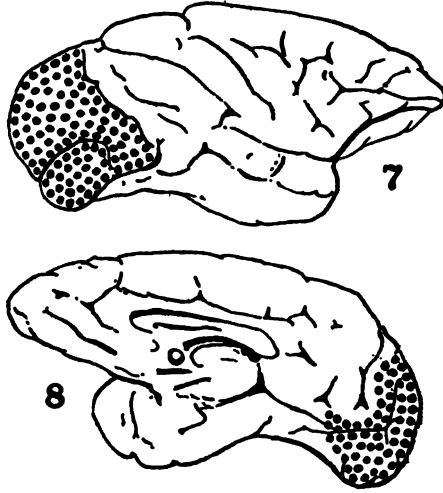
In the monkey the occipital lobe is more constant and more easily mapped out by fissures than in the human brain. It may be described as that part of the cortex on the lateral aspect almost inclosed by the parieto-occipital and the inf. occipital fissures, and as the part on the mesial aspect which surrounds the calcarine fissure, bounded above by the parieto-occipital fissure and below by a line drawn from the calcarine fissure to the inferior occipital. The cortex in this area shows types of arrangement similar to those found in the human brain, and it has been found possible to map the region into at least two distinct areas, the so-called visuo-sensory and the visuo-psychic. Figs. 5 and 6 show this division, but into three areas corresponding with the three divisions of the human brain by Brodmann. It will be seen that the calcarine type of cortex occupies a much larger area than in the human brain, not only relatively to the size of the whole cortex, but also absolutely larger than the same type in man if we disregard the infolding of the calcarine fissure. Brodmann has



FIGS. 5 and 6. Distribution of the various types of cortex in the occipital lobe of the monkey (*Cercopithecus*). Same designations for the areas as in figure 1. Adapted from Brodmann, and reduced.

not illustrated the extent of this area in the Macaque monkey, but only in the *Cercopithecus*, the brain of which closely resembles that of the Macaque. Mott has given illustrations of the Macaque brain with the distribution of the calcarine type of cortex, which are here reproduced (figs. 7 and 8) for comparison with that of the distribution in the *Cercopithecus* by Brodmann. The findings of Mott (50) differ very much from those of Brodmann in the allied family. He does not illustrate the extension of the so-called visuo-psychic type of cortex and it is, therefore, impossible to make a full comparison with the findings of Brodmann. It may be said, however, that the examination of the brains of the Rhesus monkeys used in the present work has shown a distribution of the calcarine type of cortex more nearly like that

found by Brodmann in *Cercopithecus* than that illustrated by Mott in the Rhesus.³



FIGS. 7 and 8. Illustrating the distribution of the calcarine type of cortex in the Macaque monkey (*Macacus Rhesus*). Adapted from Mott, and reduced.

From the resumé of the work of the different histologists it will be seen that the occipital cortex can be roughly divided into two areas, of somewhat different structure, and that one of these areas is very definite. The anatomo-pathological studies indicate a close connection between the occipital lobes and the eyes and this connection is admitted for the calcarine type of cortex more generally than for the surrounding regions. In the next section of this article, we shall see there is little warrant for the assumption that the cortex surrounding the calcarine type of cortex has visuo-psychic functions, although most workers in this field believe that the calcarine cortex is concerned with visual sensations.

The occipital lobe is in connection with the external geniculate bodies, the pulvinar and the anterior corpora quadrigemina.⁴

³No special effort was made to mark the limits of the other areas of the occipital cortex and a definite statement of their extent in the Rhesus monkey can not be made at present.

⁴For the description of the relations and the connections between the occipital lobes and the retinae, see especially von Monakow (45, 47 and 48), Dimmer (13), Knies (38), Moeli (44), and Mott (49).

THE LOCALIZATION OF FUNCTION IN THE OCCIPITAL LOBE

Movement. Electrical stimulation of the occipital cortex results in movements of the eyeballs and of the intrinsic eye muscles. The eyeball movements are associated movements. Different observers have located in this region centers for a number of movements, and have demonstrated a functional connection between this region and the precentral cortex. It has been found that the eye movements occur upon stimulation of the occipital lobes even after the frontal cortex has been destroyed, and that they also occur upon stimulation of the frontal cortex after the occipital lobes have been extirpated. This has been taken to mean that there are independent centers for the movements of the eyes; one in the occipital lobe, the other in the cortex anterior to the central fissure.

Ferrier (14) reported that in monkeys stimulation of points on the angular gyrus produced movements of the eyes "to the opposite side, with an upward and downward deviation, according as the electrodes are placed" anterior or posterior to the parallel fissure. He has also noted the contraction of the pupils and the tendency to closure of the eyelids, and occasionally an associated movement of the head to the opposite side. Ferrier's observations have not been confirmed by other observers, and Schäfer (62, 64) and others assign to the occipital lobes this motor function. Schäfer reports: "If the electrodes are applied directly to the upper surface of the occipital lobe, conjugate deviation to the opposite side with downward direction of the visual axes is extremely well marked, and it is also produced by excitation of the upper part of the mesial surface of that lobe, and of the quadrate lobule immediately in front of the internal parieto-occipital fissure. If, on the other hand, the posterior extremity of the occipital lobe, its lower or tentorial surface, and the posterior and lowermost part of its mesial surface be stimulated, the lateral deviation is combined with an upward movement. In both cases the downward or upward movement may be almost uncomplicated by lateral deviation; this depends upon the position of the electrodes. On the other hand, there is an intermediate zone, narrow on the mesial surface of the lobe, and broader upon the outer or convex surface, excitation of which

is productive of simple lateral deviation. All the effects are strongest upon the mesial surface, especially toward the anterior limit of the lobe; it is here that the local point for the movements is located." On the assumption that the eye movements resulting from stimulation of the occipital lobes are due to something akin to sensations Schäfer (64, p. 5) was led to conclude that the occipital lobe has the following physiological connections: "The whole of the visual area of one hemisphere is connected with corresponding lateral half of both retinae. The upper zone of the visual area of one hemisphere is connected with the upper part of the corresponding lateral half of both retinae. The intermediate zone of the visual area is connected with the middle part of the corresponding lateral half of both retinae." Von Bechterew (2) has located only three centers for movement of the extrinsic eye muscles, and he has reported that eye movements may be obtained from stimulation of the parietal lobe as well as from the stimulation of the occipital. It is worthy of note that Steiner (70) has found the occipital lobe unresponsive at birth, although at this time the stimulation of the frontal (pre-Rolandic) cortex results in eye movements. This has been confirmed by Berger.⁵

Many observers have recorded movements of the iris, i. e., pupillary changes, following the stimulation of the occipital lobes. Von Bechterew has located in the occipital lobes two centers for pupillary movements, a contraction center in the middle part of the occipital lobe, and a dilating center slightly inwards from the contraction center. These results or similar ones have been found by other observers, and they are partly confirmed by the observations on man when there is disease or destruction of the occipital cortex.⁶

The observation of Schäfer (66, p. 750) on the latent periods of the movements from stimulation of the occipital lobe and of the precentral cortex are of primary importance, and have often

⁵ See also Schäfer (62), Gerwer (22), Mott and Schäfer (51) and Obregia (57).

⁶ On the pupillary changes see also the following articles: v. Bechterew (2, 3); Parsons (59); Mislavsky (43); and also Alamagny (1); zur Verth (72); Bernheimer (6); Leyden (39).

been overlooked by other investigators. He has carefully timed the movements and finds that the latent period for movements from occipital lobe stimulation is much longer than that from the stimulation of the Rolandic area. That this lengthening of the time is not due to the passage of the impulse to the frontal lobe, i. e., to the motor cortex, and thence to the muscles is shown by the fact that the movements from stimulation of the occipitals may be obtained after the frontal lobes have been excised. We are, therefore, compelled to conclude that the cells in the occipital lobes have a more direct connection than that through the motor cortex, although it is apparent from the results of Schäfer's work that there are more intercalated cells in the chain from the occipital lobes to the eye than from the pre-Rolandic cortex to the same muscles.

Sensation. It was known for a long time that destruction or injury of parts of the cerebrum caused visual defects or disturbances, but the recognition of a localized center or centers for visual sensation and perception has been largely due to the results of experiments on animals. In the hands of different observers the method of extirpation has led to somewhat discordant results, and to opposing conclusions. The controversies regarding the functions of the occipital lobes will not detain us here, but although they apparently did not change the opinions of those who took leading parts therein they have left us many observations of value.

Extirpation of certain cortical areas in the dog's brain led Munk to the conclusion that in this animal there is a center (designated by him as A1) for visual perception and that the surrounding more extensive region was a subsidiary visual center which could be used when the primary center was injured or destroyed. Munk (52-56) furthermore attempted a more careful localization and attributed to parts of the center more definite functions. The conclusions of Munk were strongly combatted by Goltz who at first denied there was any localization in the cortex of the brain, but who later admitted that a certain amount of localization was possible, and in fact was indicated by the results of the experimental investigations. The position taken

by Goltz (23-25) in regard to localization of vision was that although a visual defect, which he did not deny, might be produced by the ablation of part of the cerebral cortex, the defect was not due to the extirpation of the cortical cells concerned with the visual processes, but to a general effect and might be produced by the extirpation of parts of the brain other than that assigned the visual function by Munk. Regarding this he has written: (23) "Indem ich also auf Grund meiner neueren Erfahrungen einen grösseren Einfluss des Hinterhirns auf das Sehvermögen für festgestellt erachte, kommt es mir dabei nicht in den Sinn, etwa eine begrenzte Sehsphäre zuzugeben, wie sie Ferrier, Munk und Luciani konstruiert haben." This admission of the possibility of localization is much the same as that of Loeb (40-42) who investigated the so-called visual cortex at a later date, and who has taken a stand much like that of Goltz. Loeb asserts that the visual disturbance following operations upon the occipital lobes which is temporary in character is due to a lowering of the irritability of the retina-cerebral cortex mechanism. The differences between the Munk and Goltz school does not, however, disappear even if we consider that the Goltz and Loeb explanation of the latter be correct for the symptoms that are temporary. Part of the difference between the explanations of Munk and Goltz rests, as von Monakow has shown, upon different standpoints in the collection of the data. Goltz has used as evidences of the presence of visual sensations the movements of the eyes and of the related parts when the eyes are stimulated by light, but there is no reason to believe that these reactions are more than reflex in character, and there is good reason to believe that they do not depend entirely upon the activity of the cerebral cortex. It would be equally correct to say that there was no localization of movement or of the skin sensations because it is possible to get reflexes of the leg after there has been complete separation of the spinal cord from the cerebrum. Loeb does admit, however, that the destruction of the occipital lobe in man is followed by a blindness, which he believes is due to a loss of irritability on one side of each retina, but he explains the results in dogs to be not a loss but only a decrease

in irritability in that organ.⁷ He furthermore asserts that, with Goltz, he believes the associative memory of animals deprived of their occipital lobes does not differ from that they had previous to the operation. The main difference between Goltz and Loeb and the followers of Munk, Hitzig, and others, is in the conception of the relation of the brain to mental processes. The former object to the interpretation that extirpations of or injury to parts of the brain cause psychical disturbances, while Ferrier, Munk, Hitzig, and other investigators interpret the defects to be psychical. From the standpoint of localization, which apparently all admit in some form, the important question is not concerning the presence or absence of certain psychical disturbances but the definite or indefinite restriction of certain functions to certain areas. There can be no doubt that in man certain cerebral destructions produce certain effects of the nature of loss of associations, e. g., those of speech, and the clinicians still debate whether or not these losses mean mental deterioration. Whether or not the disturbances are to be interpreted as psychical will depend upon the connotation of the word psychical.

The conclusions of Munk were supported by von Monakow (46) both from the results of personal experiments and from the examination of the brains of animals upon which Munk had operated. von Monakow, however, believes the visual area is not as small as it was pictured by Munk, and considers that, in dogs at least, it extends anteriorly beyond the limits assigned by Munk.

The conditions reported by Hitzig (30-34) following removal of parts of the occipital lobes in dogs was that of crossed amblyopia. This is unlike the results of other investigators, who have reported a condition of hemianopia (for the opposite visual field) following the extirpation of cortical regions in both dogs and monkeys. Hitzig had also been led to conclude that a similar condition may follow lesions of the anterior part of the brain, but these results have not been confirmed by independent investigators. As an explanation of Hitzig's conclusions it has been pointed out that the visual defect in the dog following the extir-

⁷ Why he has selected the eye in this connection is not made clear, for it would seem that the explanation should be made in regard to the central nervous structures.

pation of one occipital lobe is blindness, not for the opposite eye, but for objects in the opposite field of vision, which on account of the large number of the optic fibers crossing at the chiasm is more marked (i. e., includes more than half of the visual field) in the opposite eye in the dog. Perimetric examinations of animals have shown that from two-thirds to three-quarters of one retina in the dog is connected with the homonymous occipital lobe. The conclusions of Hitzig are, therefore, taken to indicate that on account of the involvement of such a proportion of the retina of the one eye by an unilateral occipital lesion the exact nature of the defect was not discovered, i. e., there was retention of vision for only about one-quarter of the field and the appearance was that of a complete blindness for the whole field of that eye.

Using monkeys as subjects, Ferrier localized the visual area at first in the angular gyrus, but later concluded that the occipital lobe was used in connection with the angular gyrus, and called the area the "occipito-angular" area. The later conclusions of Ferrier were due to the criticism of his early work by Schäfer and others, and to the fact that he repeated the work using somewhat different methods. His early experiments were performed without aseptic precautions, and it is this condition and its resultant consequences (e. g., the secondary extension of the injury by infection, etc.,) which Ferrier uses to explain his first results and conclusions. In the later localization work by Ferrier (14, p. 268-305) the angular gyrus is given a prominent place and he reports that the occipital lobes may be cut away alone without appreciable impairment of vision. The visual defect reported by Ferrier (14, p. 273) was that of amblyopia. Many of the animals operated upon by Ferrier showed a blindness which persisted for only a few hours. This has been urged by others as a criticism of Ferrier's work and conclusions, for it has been said that the observations taken so soon after the completion of the operation do not warrant the conclusion of blindness. Although Ferrier rightly retorts that the "period of reliable observation is not to be measured by the mere time that has elapsed since the operation, for the period of recovery is most variable, many animals being up and active almost before their wounds have been

dressed," it does appear to stretch a point to conclude that the blindness may remain for only a few hours and then entirely disappear. In reply to Schäfer, the citation of cases from his work in 1884 is not convincing in regard to the part played by the angular gyrus in visual perception or sensations (15).

Following are abstracts of several cases cited by him in the discussion with Schäfer: In one monkey the left angular gyrus was cauterized. The left eye was secured so that the animal could not use it for vision. A half hour after the operation the animal was apparently "wide awake but would not move unless touched"; it sprawled on the floor and knocked against obstacles; an hour later it bounded away when it was touched and ran against the leg of a table, but did not show signs of perception when it was approached cautiously without noise; on the following day "no defect of vision, amblyopic or hemiopic, could be detected." In regard to the effects of similar lesions on other animals he says: "Similar results were obtained in many other instances, and if in some they were even more transient, this may have been due to less extensive destruction or perhaps differences in animals as to the relative importance of this region in the ordinary exercise of their visual function." Three cases cited by him are to be compared with the results in the present work. In these three animals the occipital lobes were destroyed or severed with great care so that the angular gyri were not injured. In one the occipital lobes were exposed and the surfaces were "destroyed by cautery, which was also passed deeply into the interior of the lobes so as to cause as much disorganization as possible. Care was taken not to injure the angular gyrus." About forty minutes after the operation the animal began to move but staggered a good deal; its eyes were open and the pupils were dilated. An hour and a half later there was "emphatic evidence of sight; ran away as I approached it, carefully avoiding obstacles"; it entered its cage, and avoided a cat that had taken up its quarters there; "tried to escape my hand when I offered to lay hold of it, but picked up a raisin which I had left on the perch." In a second animal the occipital lobes were severed "by a perpendicular section with hot wires about a quarter of an inch posterior to the parieto-occipital fissure, so as to avoid all inter-

ference with the angular gyrus;" in twenty-five minutes, it was observed that the animal "can see quite as well, as it avoids obstacles, and when removed, regains its position by the fire." In a third animal "both occipital lobes were severed with a galvanic cautery and scooped out bodily; the line of incision in both cases passed between the anterior extremity of the first occipital and the parieto-occipital sulcus;" in half an hour the animal sat up and wanted to move about; for two hours there were no definite tests of vision, but at the end of that time "a piece of apple was thrown into the cage, and though it fell a full arm's length away, the animal, without the slightest hesitation or want of precision, put forth its left hand, picked it up and ate."

Ferrier explains the results of Schäfer and of Munk on the ground that both of these investigators cut away more than the occipital lobes and that in their experiments the angular gyri were injured. The greatest difference, however, between the conclusions of Ferrier and Schäfer is that the latter believes the blindness is of the nature of an hemianopsia. The results obtained by Schäfer, especially those in conjunction with Sanger Brown, are apparently convincing not only in regard to the participation of the occipitals in vision, but also in regard to the character of the defect following lesions in these regions (67). Schäfer (63, 65) not only obtained results on the occipitals opposed to those of Ferrier but has failed to obtain the positive results on the angular gyri reported by Ferrier to be constant. In fact, Schäfer has shown that the angular gyri may be removed without interfering with vision in the least, but that when these parts are removed and the underlying fibers (going to the occipital lobes) are injured, some visual disturbances are to be noticed. Some of Schäfer's results are particularly instructive. One animal upon which he operated, extirpating the angular gyri on both sides, showed no loss of vision, no defect of movement of the eyes, and no anesthesia of the conjunctiva or the cornea, and although this animal was kept alive for several months, no disturbance of the eyes, visual or otherwise, was found. In this connection Schäfer concludes: "A single well marked negative case like this is conclusive against the idea that in the monkey cerebral visual perceptions are localized in the angular gyri" (63, p. 365). On the

other hand the positive results following extirpations of the occipital lobes are worthy of note. One animal, the left occipital of which was completely removed showed a bilateral homonymous hemianopsia, persisting for the whole time the animal remained alive (eight months) (63, p. 367). Another monkey from which both occipitals were removed showed a "total and persistent blindness. The animal could only find food by groping and smelling. Brought into a strange place, it ran against every obstacle. Placed in a dark room and with a light flashed upon it, no signs of perception were given. Hearing was very acute and all other senses besides vision were unimpaired" (63, p. 368-9). This animal was clinically in the same condition as a monkey observed by Ferrier and Yeo, from the brain of which they removed both occipitals and both angular gyri. Other cases were cited which confirm this general conclusion of the non-participation of the angular gyri in the same sensory processes. In conclusion Schäfer says that for complete blindness the "removal of the lobe must be complete and when a small portion of one of the lobes is left, although blindness is not complete, yet the limit of the visual field of the retinae may be greatly restricted" (63, p. 370).

Schäfer sums up the differences between him and Ferrier as follows: "The chief points at issue are (1) the connection of the angular gyrus with central vision of the opposite eye; (2) the relative importance of the occipital lobe. According to Dr. Ferrier's experiments, decortication of the angular gyrus of one side produces blindness of the opposite eye (amblyopia); of both sides produces blindness of both eyes. According to our experiments, decortication of the angular gyrus of one or both sides is not necessarily followed by any visual defects perceptible to our means of investigation in animals; but complete eradication of the gyrus produces hemiopia (not amblyopia), which is temporary only. According to Dr. Ferrier's experiments, destruction of one or both occipital lobes alone produces no appreciable effect whatever on vision. According to our experiments—which are merely confirmatory of those of Munk—removal of the occipital lobe only, without the angular gyrus, produces permanent hemiopia; of both occipital lobes, blindness of both eyes

which is also permanent and, so far as we were able to judge, complete" (65, pp. 158-9). Schäfer admits the possibility that the visual area does extend beyond the limits of the occipital lobe and asserts that the cortex undoubtedly extends somewhat anteriorly beyond what is regarded the limits of the occipital lobe, especially on the under surface (and perhaps also the mesial surface).

The conclusions of Munk, Goltz and Hitzig, which were drawn from observation of dogs deprived of parts of the cerebrum, can not be applied directly to man, whose brain it has been the object to understand by the experiments upon animals. On the other hand, the results of Ferrier and of Schäfer are almost directly applicable to the brain of man, but in addition to the experimental results on animals there are numerous clinical observations on man of the visual and other sensory conditions following destruction of parts of the brain, and the results of these observations are of primary importance. Chief among the results that have been reported are those of Henschen (27, 28). This author examined all the available accounts of reported cases of blindness following lesions of the brain, and contributed observations of personal cases. From the consideration of the clinical accounts and of the pathologico-anatomical examinations he concludes that the visual center is limited to the cortex of the calcarine fissure, and that the centers for light and color coincide. He furthermore defined an area, in the anterior part of the calcarine fissure, to which he assigned a connection with the macula lutea, and disputed the conclusions of others who located the macular center in the cuneus. Regarding the lateral surface of the occipital lobes and the cortex of the angular gyrus, he says "I have not found documents which prove there exists a projection in this part, and a lesion of this part does not cause blindness" (28). This part of the cortex is, however, admitted by him to have something to do with vision and this fact leads to the conclusion that there are two areas: one restricted, in which there is a "projection of impressions," while the other, extracalcarine area, does not receive the impulses directly, and is more extensive. The calcarine area is conceived to be a sort of brain retina while the extra-calcarine area is supposed to have perceptive functions.

Vialet (73) using similar material concluded that the visual center extended beyond the calcarine fissure, including the cuneus, the lingual and the fusiform lobules, and part of the occipital pole. He concluded that lesions of these parts separately may be accompanied by hemianopsia, but Henschen has analyzed the cases of Vialet and has assured himself that the results obtained by the latter are in harmony with his own hypothesis. Against the hypothesis of Henschen, von Monakow has brought forth the fact that in cases of early blindness with total destruction of the optic nerve, where we expect to find the cortex wasted in the corresponding brain areas, the principal atrophy is to be observed over the convexity of the occipital lobes as well as along the calcarine fissure. The conclusion of von Monakow has recently been supported by Wehrli (77) who has examined histologically the brains in cases of blindness. He supports the view that the fibers from the macula lutea spread over a wide area at the occipital pole, and that the visual area is widely distributed and not to be limited to the area surrounding the calcarine fissure, not even to the part of the cortex with the cell lamination known as the calcarine type. He claims, furthermore, that purely cortical lesions causing hemianopsia have never been observed.

There are many cases reported clinically which seem to indicate that the localization of visual processes is not as limited as Henschen believes, but it must be said that the observations have often been poorly made, not only clinically but also pathologically, and it is difficult to estimate the value of the individual contributions. Of the negative results with the experimental method, the work of Vitzou (74-76) is of interest. He extirpated parts of the occipital lobes and of the angular gyri and found the animals lost their visual ability immediately after the operation, but recovered it to a great extent during the time they lived. In one animal he extirpated the occipital lobes but preserved the angular gyri. Although this animal was blind after the operation, this condition ameliorated in a month and a half, and after twenty-one months the animal was able to recognize its cage and people. A second monkey, in which both angular gyri and both occipitals were extirpated, was completely blind for about two months but following this period it began to recognize objects and persons. In a

third monkey, both occipitals and parts of both angular gyri were extirpated. This operation resulted in complete blindness for about two months, but at the time the report was made, about one year and three quarters after the operation, the monkey recognized objects and people. This animal is reported to have been able to see better than either of the two previously described animals. In a fourth monkey both occipitals and the posterior parts of both angular gyri were removed, causing complete blindness, which was much improved at the time of the report, twenty-one months after the operation.

Somewhat similar negative results have been reported by Panici (58) from the laboratory of Luciani. The conditions of three monkeys are reported, in which various operations were performed. In one animal the decortication of the posterior segment of the right calcarine fissure produced a bilateral homonymous psychical hemianopsia which persisted for only two days. In a second monkey Panici first extirpated the posterior part of the left cuneus. This resulted in no defect of vision, but following a second operation, in which additional cortex of the left occipital lobe was extirpated, there was found a condition similar to that in monkey 1, but which lasted only three days. Seven days after the second operation, Panici extirpated the whole left occipital lobe, and found a bilateral homonymous psychical blindness for about a month, which condition gave place to an amblyopia. A month later he extirpated the whole of the right occipital lobe, and found this to result in a right psychical hemianopsia for about a month, after which time the animal began to react to food, but remained indifferent to gestures and to a mirror, but with a corresponding amblyopia. At the first operation on a third monkey the internal portions of the right and left cuneate lobes were extirpated, and a week later this was followed by a more extensive extirpation. Neither of these operations appeared to have any effect upon the visual ability of the animal, but after the third operation, three weeks after the second, the complete excision of the left occipital was accompanied by a bilateral homonymous psychical hemianopsia, which continued for about a year. Twelve days after this operation the right occipital lobe was completely severed, which produced a complete

psychical blindness for one year. Thirteen months later a final operation on the left produced complete blindness in the right visual field, and three and a half months subsequent to the operation on the left the right hemisphere was destroyed up to the fissure of Rolando. After the extensive destruction just noted the animal became blind in both eyes, over the whole field.

These results of Panici are of very great interest for they show how much of the brain must be destroyed to produce a complete blindness, and also because they show how transient some of the visual defects may be. It will be seen in the experimental part of this article how closely some of the results of Panici resemble those observed by me.

By the method of recording electromotor changes in the brain, Danielewski (12) and Gotch and Horsley (26) found the occipital lobes responded to stimulation of the retina and the optic nerve.

After considering the various facts Schäfer (66) has summed up regarding the occipital lobes in man as follows: "There is abundant clinical evidence to show: That lesions of the occipital lobe produce disturbances of vision which are invariably of a hemiopic character. That lesions of the mesial surface of the lobe in the immediate neighborhood of the calcarine fissure are those which for their size produce the most serious visual disturbances. On the other hand, lesions of parts near to but not involving these parts of the occipital lobe have not infrequently been associated with defective appreciation of visual objects and particularly with word blindness. Perimetric observations in man show that in cases of hemiopia, produced by lesions of the cortex (of one hemisphere), the line of demarcation between the blind and functional parts of the retina usually passes, not through the middle of the fovea, but on the homonymous side, in other words, the fovea is not involved in the hemiopic condition." These, and the observations of animals following experimental lesions have led him to the following conclusions: "The whole of the visual area of one hemisphere is connected with the corresponding lateral half of both retinae. The upper zone of the visual area of one hemisphere is connected with the upper zone of the corresponding lateral half of both retinae. The lower

zone of the visual area of one hemisphere is connected with the lower zone of the corresponding lateral half of both retinae. The intermediate zone of the visual area is connected with the middle zone of the corresponding lateral half of both retinae. The focal point of the visual area, which is placed on the anterior part of the mesial surface of the occipital lobe, is connected with rather more than the corresponding half of the macula lutea of each retinae." With these conclusions von Monakow (47) is not in complete accord, although he considers the visual areas to be rather widely spread, and not be located in a small area.⁸ He believes the visual area for sensations extends beyond the calcarine area, and includes the lateral part of the occipital lobes, the cuneus, the lingual lobule and the gyrus descendens. More than this, von Monakow admits that the subcortical parts may have something to do with visual sensations (as mental phenomena), and if this be so, it would explain many of the apparently discordant observations and conclusions of experimenters and clinicians.

Throughout the previous discussion the psychological conclusions regarding the experiments upon animals and the observations of man have been reported as if they were of about the same character. This is not so, for we find the results have been interpreted to mean that the occipital lobes are the sensory receiving stations, or, on the other hand, that these portions of the brain are the places in which the visual associations (physiological and psychological) take place. From the first point of view the visual disturbances are considered to be inability to see, from the second they are conceived to be inability to perceive. This is the main point of difference between the adherents of Goltz and those of Munk. With the methods used by these investigators a solution of the problem is not possible, and the results which have been reported to us may be interpreted in either way.

Perceptions, etc. Regarding the psychic or higher intellectual functions of the occipital lobes little need be said. In an area close to the occipitals some have located the visual speech func-

⁸ "Diese und andere Beobachtungen hatten mich zu der Annahme geführt, dass die Stelle des deutlichsten Sehens überhaupt nicht in einer engen corticalen Zone repräsentiert sein könne."

tion, but others have placed this same function in the angular gyrus, to which it will be remembered Ferrier assigned such important visual functions. Optical agnosia, alexia and agraphia have all been located in the occipital lobes, but usually the location has been so indefinite that the individual localizations can not be discussed here. It may, however, be said that the more definite localizations are localizations in areas of the cortex and of the underlying white matter which can not be considered part of the visuo-sensory or of the visuo-psychic types of cortex. An examination of the diagrams of von Monakow and of Moutier in comparison with the cortical differentiation of Brodmann, of Campbell, of Bolton and of Elliott Smith will make this clear.

EXPERIMENTAL.

INTRODUCTION

On account of the lack of information in regard to the functions of the areas (visuo-psychic) believed by histologists to be closely connected with the visual center (calcarine area), it appeared advisable to attempt to determine the functions of this so-called psychic area, and it was this problem which originated the present work. Subsequent developments directed the inquiry into other channels, and eventually it became imperative to reinvestigate the whole occipital lobe. Only part of the work has been finished but sufficient has been accomplished to indicate that the histological differentiation has not the physiological import which the histologists impute to it. The present work deals mainly with the relations of the lateral aspect of the occipital lobes in the monkey to certain sensory and perceptual states.

In this work eight monkeys were used, each of which was trained in visual discrimination previous to the extirpation or the destruction of parts of the occipital cortex. These animals were purchased from a New York dealer, and, as far as could be ascertained, had not been used for experimental purposes or for pets before the time of purchase. All the animals were reported to be about six to nine months old at the time they were received, although in size there was a considerable variation. During the nine months following their purchase they were utilized by Dr. W. T. Shepherd in psychological investigation, and after that period they were entirely in my charge. During the time they were under my observation they were fed mainly by me, but at other times by three other people. The animals were, therefore, well acquainted with the experimenter and they acted in as normal a manner as could be expected from animals kept in captivity. The observations of the animals continued for about fifteen months, at the end of which time all had been operated upon and killed. The animals lived, therefore, approximately two years, during which time they were under almost constant observation,

although the experiments to be reported in the present paper were not being conducted throughout all this period.

Two of the animals upon which operations were performed were shown at meetings of scientific associations, monkey 7 at the meeting of experimental psychologists in April, 1910, and monkey 1 at a meeting of the Georgetown University Clinical Society in February, 1910.

During the time the animals were under my observation they were kept in pairs or by threes in cages, 114 cm. high, 90 cm. wide, and 58 cm. deep. The front and right hand ends of the cages were covered with chicken wire, of one and one-half inch mesh. The top, the bottom, the back and the left side were boarded. Within the cage there was a shelf 30 cm. wide jutting from the back of the cage at a height of about 45 cm. This shelf extended the width of the cage. From the front of this shelf to the floor of the cage was a sliding door. When an experiment was to be conducted one of the animals (the one not working) was induced to get below the shelf, the sliding door was closed behind it and the monkey confined beneath, leaving the second animal free so that the experiments might be conducted without interruption and without disturbances.

The sliding door was kept open except at the time of the experiments, and the animals, which lived in these cages during the progress of the experiments, ran in and out of this space as they wished. There was, therefore, no cause for alarm on the part of the animal which was confined. The animals were not handled and the environment, except for the presence of the experimenter, was kept fairly constant throughout the day. Food and water, other than that used in the experiments, were pushed through or under the wire front of the cage and the animals took as much food as they wished.

Notwithstanding the animals were not handled, all became fairly tame, they would hang upon the wire netting of the cage and take fruit, nuts, bread, etc., directly into the mouth, and at no time did they appear to be disturbed by the presence of the experimenter.

In the accounts of the work which will be found in the section dealing with the observations of the animals before and after the

operations, it will be found that there are many expressions similar to those used in describing the actions of man. It must not be understood, however, that these expressions are used in the same way they are used for man, but solely for convenience and for brevity. It would, perhaps, have been advisable to describe the actions of the animals, but such descriptions to be intelligible must be accompanied by equally minute descriptions of the situations, and from the visual side can best or accurately be shown only by means of motion pictures. It must be understood, however, that the anthropomorphic method of describing animal activity is not to be taken as an indication that the animal had the same sort of mental process which a man would have under similar circumstances, but even the most careful of the animal psychologists have found it impossible to dispense with terms descriptive or indicative of mental processes in man. It would, for example, have been better to say "the animal turned its head and its eyes in different directions" rather than "the animal peered around the cage" but, on the other hand, with the warning that has been given, no one should interpret the animal activity in a way other than in terms of movement.

METHODS

Training. To one who is familiar with the studies on the functions of the cerebrum, it is evident that many of the differences in opinions and in conclusions have been due to the fact that the tests of animals upon which experiments have been made have been of such a character that they permit of more than one interpretation. It is certain that simple observation of animals following extirpation of parts of the cortex or following the injury to other parts of the nervous system is an inadequate method. For the reason that both the description of results of previous physiological work on the association areas were lacking in exactness and definiteness, about ten years ago, the writer (16-20) devised a method to determine more accurately the sensory and associative functions of parts of the nervous system, especially of the cerebrum. The first investigation to be performed by this method was one on the association functions of the frontal lobes (16, 17), but the method has been applied to

sensory functions both in the present work and in work published by others. This method has previously been described in various places and at present it may be said that it depends upon the formation of definite associations in an animal and the determination of the presence or absence of the associations after lesions of the nervous system. It will be readily appreciated that by this method there is an approach to the method the clinicians use in the examination of their human cases, and there is afforded the possibility of obtaining very exact information regarding the sensory or associative processes of animals even though in animal experimentation we are hampered, as compared with the investigations on man, by the lack of the definite means of communication by speech.⁹

Before an animal is operated upon we must become acquainted with its mental character, we must know the animal well before we operate so that we shall be better able to notice deviations from its normal mental character, following, let us say, the loss or the destruction of a certain amount of a certain part of the brain or the spinal cord. Usually it is not possible to keep a laboratory animal for a period of months or years in order to

* This method was used to a limited extent by a number of investigators before it was applied by me to the investigation of the frontal lobes, but all who had previously used it had employed it in much the same way as they made their regular observations. From the account given by Kalischer (36) of an investigation of the functions of the temporal lobes by this method, it might appear to one unacquainted with the situation that he were the originator of it. He calls it "meine Dressurmethode." To an impartial observer it appears a mere hair splitting matter to differentiate Kalischer's method of testing hearing in dogs from that of Thorndike (71), but throughout all of Kalischer's work there is evidence of total ignorance of the work which has been performed in America on animal psychology. It is of interest to note that following a critical note by me (20) Kalischer (37) relinquishes claim to the method as a whole, but makes a claim that he has used the most simple movement as an indication of the presence of sensation in the animal. This claim, in the way it is made, I am sure no comparative psychologist will admit, for the movement of the paw, or the movement of walking, is just as simple physiologically as the movement which Kalischer has used, viz., that of the snapping up of food, which must have associated with it other movements of the head. It is also of interest to note that Kalischer admits in his second article (37) some acquaintance with the training method that had been used by others (e. g., Gaule, 21), although no mention is made of this in his first article.

get acquainted with its mental make-up, and to facilitate matters we have at our command the training method devised by Lloyd Morgan and so ably followed out by Thorndike (71) and many others. By the use of this method we may have animals, which are to be operated upon, form certain associations, the presence or absence of which can be later determined by the experimenter in a few minutes. The associations always involve two elements, the sensory or afferent, and the motor or efferent. It is believed that in most, if not all, cases a third element is present, viz., the associational. By proper combinations and by the formation of more than one habit at a time it is possible to differentiate these three elements, and to infer without question which of the elements is lacking and which has been interfered with.

Comparative psychologists have investigated the visual discrimination ability of monkeys, but some have not been satisfied from the results and the methods of the experiments that monkeys discriminate colors as such. The methods of other investigators and the results obtained by them indicate that monkeys have good visual discrimination, although a few are unwilling to admit the conclusiveness of the experiments on color. It seemed advisable to have the animals acquire a habit indicative of color discrimination but none of the methods previously employed appeared to be sufficiently well accepted and none appeared to place the animal in a position similar to that it might normally be in. In the present work an attempt was made to have the animal tested in as normal a way as possible, and to form the habit or to produce the discrimination quickly as well as naturally.

In the present work it was necessary to have the animals form habits of a definite visuo-motor character. For this reason the sensory part was made as simple as possible, viz., foods of different colors, which had to be discriminated. In a series of experiments on the color discrimination of monkeys, conducted by Dr. W. T. Shepherd (68), under my direction it was found that the animal rapidly learned to discriminate colored objects if the stimuli were of a character to attract and to hold the attention. In most animal experimentation it has been found difficult to get stimuli which will attract the attention of the animal, stimuli which are appreciated by the animal and which will lead

to definite reactions. In color experiments it has been customary to use the box method of feeding, the box being covered with colored paper or lights being exposed near the box. The employment of this method has certain advantages and certain disadvantages that are apparent to all who have used it. On the one hand the colors may be changed to a considerable degree both in intensity and in hue, but on the other hand the use of papers, filters, or other similar means of giving stimuli introduces an element foreign to the animal mind and must have less effect than the exhibition of colors in direct connection with the object to be obtained. Moreover, in order to determine discrimination ability many investigators have found it necessary to punish the animal for a wrong selection and to reward it by means of food for a correct selection. In most of the work which has been reported the punishment has also been something foreign to the animal mind, and the use of electric shocks and other similar methods to inhibit the activities of an animal have led at times to disturbances in activity which are not conducive to proper experimentation.

Because of the difficulties and disadvantages of the methods which have been previously employed, colored food was used in the experiments of Dr. Shepherd, and in these experiments it could not be doubted that the attention of the animal was directed toward the stimuli. In the early work bread or rice was mixed with appropriate colors, two or more differently colored pieces of food were simultaneously displayed before the animal and the animal learned to select one or more of the colored pieces. The artificial method of punishment was discarded in these experiments and a more natural method was employed. In the method the punishment and the cause of the inhibition of the wrong reactions were contained in the stimuli and were not extraneous. Certain of the colored foods were soaked with a solution of quinine and others had added to them a certain amount of saccharine. On the assumption that the monkey likes sweet and dislikes bitter tasting foods, we should find that the animal would learn to decline the bitter food and to take only those which were sweet or at least not bitter. As a matter of fact, this assumption was found to hold in almost all the animals tested. With only one

monkey was there any difficulty in the establishment of the habit to take the properly colored and sweetened food and to leave the food which had been made bitter. This animal seemed to have little objection to the bitter taste and for a relatively long time ate both the sweet and the bitter foods. This animal died before the effects of the operation were determined and the records are not included in the accounts of the experimental work. Eight other monkeys, however, had a decided dislike for bitter food, or to express this in terms of reaction, these animals soon learned to permit the bitter foods to remain on the plate, and although hungry, did not touch them. In the formation of the habit and in the subsequent tests following the operation the monkeys were not kept in a state of absolute hunger, for it was found by previous work that this factor would interfere with the normal working of a monkey.

In performing the experiments to be described here the animal was in its usual cage. The cage has already been described. The animal sat upon the shelf, sometimes near the wire netting on the right hand side and sometimes farthest away from the netting. A glass plate, 12.5 by 18 cm., was arranged on a platform with cup hooks to catch the wire of the cage so that it could be at-

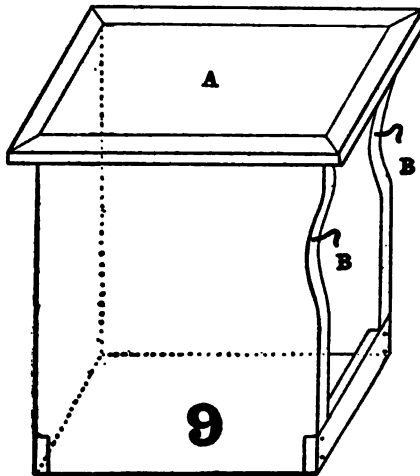


FIG. 9. Glass plate and holder for the presentation of food to the monkeys. A, glass plate; B-B, cup hooks to catch upon the wire netting of the cage.

tached to any part of the front or side of the cage. The arrangement was such that the glass plate was kept horizontal. Fig. 9 illustrates the glass plate and the holder. All this part of the apparatus was colored black. The food was placed upon the glass plate, the holder was arranged on the cage and the experimenter moved away from the cage about a meter's distance. The animal, which usually sat at the farthest end of the platform, moved forwards and took the food which was presented to it on the plate. In performing the experiment in this manner the greatest possible freedom was accorded the animal, and the movement to be performed was as simple as possible and one which would give the right to conclude that discrimination did or did not take place, i. e., the taking or leaving of the food. The arrangements at a later date were made more simple, as far as the adjustments are concerned, in that the glass plate holder was attached to the cage at the beginning of the series of tests and kept there throughout the series on that day. When this was done the food was placed upon the plate in the same manner as has been described. In all the series of tests the arrangements of the breads were constantly changed, the bitter being at times on the right, at times on the left, and if three or four pieces were simultaneously displayed the bitter pieces were between the sweet, or at both ends. In most of the experiments, all of those in which the animal was being trained, the two or more pieces of food were placed upon the plate at approximately the same distance from the cage netting. In later tests at times the foods were arranged indiscriminately on the plate, the sweet being farther from or nearer the cage, etc. Most of the animals were trained on the discrimination of colored foods, and one on the discrimination of foods of different sizes.

The colored food. After a number of tests it was found that white bread was most suitable both for coloring and for food. The bread was usually 24 to 48 hours old at the time it was received. The crust was removed and the bread was cut into thin slices about 5 mm. thick, then into strips of about the same width, and finally into shorter lengths making hexahedrons, approximately cubes, 5 mm. on each edge. A large number of these were cut at one time and from 30 to 100, depending upon

the number of animals under the test, were colored; the remainder were used on succeeding days. If kept uncovered the small cubes dried, but it was found that the immersion into the solutions brought them back to their original size. The cubes were dipped into watery solutions of different colors, and thoroughly saturated. The resultant colors were fairly homonymous, but did not have the smoothness of colored papers or of other similar material on account of the small bubble holes in the bread. The colored moist breads were placed in small crystallizing dishes and covered with watch glasses to prevent the entrance of dust and to limit evaporation. In this way it was found possible to make up fairly large quantities of the different colors and to keep them fresh and moist for several days.

In the general experiments on color four differently colored breads were prepared: red, blue, green, and yellow. The color solutions, formulae for which follow, were made up in large quantities, and to each solution a proportionate quantity of alcoholic solution (5%) of thymol was added. The thymol was added for two reasons; first to prevent the formation of mold in the solution and, secondly, to overcome any special odor of the solution and to substitute for the special odor that of thymol. In this way an attempt was made to equalize the odors of all the stimuli, in order that the discrimination might not take place because of the differences in odor. Particular attention is called to this factor, for although we have no right to say that by this introduction of a foreign odor (distinctly perceptible to man) the odors of all the pieces of bread were the same, I believe the strength of the solution was sufficient to overcome any odor of the individual solutions. None of the individuals (men) who were tested could detect the least difference in the odors of the solutions, and the tentative conclusion was drawn that the individual odors of the solutions were overcome, and that all the breads smelled alike.

The formulae used in making the solutions were as follows:

Red: Congo red.....	1.0 gram.
Water	300.0 cc.
Saccharine	3.0 grams.

Blue: Toluidin blue	0.5 gram.
Water	400.0 cc.
Saccharine	4.0 grams.
Yellow: Methyl orange	0.5 gram.
Water	250.0 cc.
Quinine bisulphate (0.5% solution).....	25.0 cc.
Green: Methyl orange	0.2 gram.
Smaragd grün	0.1 gram.
Water	250.0 cc.
Quinine bisulphate (0.5% solution).....	25.0 cc.

When they were intended for use, i. e., for the dipping of the bread, the solutions were diluted to one-half strength by the addition of water.

Immediately after dipping the breads the colors of the breads were compared with the colored papers in the Milton-Bradley series. On this comparison it was found that the yellow bread was approximately half-way between the yellow orange and the orange yellow of that scale, but after slightly drying the color was more nearly like the latter.¹⁰ The red bread was approximately like the red of the scale, although on account of the chemical properties of the Congo red this color varied to a greater extent than did any of the others. When the bread was dipped into the blue solution it took a color most like that of the Engine colored paper 5b of the scale, being close to the regular blue shades nos. 1 and 2. When the solution was diluted to half strength, as usual, the bread became nearly like Engine colored paper 5, or like the blue of the regular scale, although the latter was a trifle darker. The bread, after having been dipped into the green solution almost exactly matched green yellow shade 1, but on standing this color changed to yellow green shade 2, and eventually became almost like yellow green. The change in this case was probably due to some combination with the air, probably oxidation.

No special examination of the colored solutions by means of

¹⁰ The colors of the breads are given in terms of the nearest color in the Milton-Bradley scale, but the comparison is rough, and in the case of green 2, the color scale showed nothing to correspond closely. The color of green 2 was much darker than the M-B green shade no. 2, but there is nothing in that scale to give a better idea of the color and shade. The difference in the colors, is, however, the main fact to be remembered.

the spectroscope or by the photometer was made, because all the colors are standard colors, and can readily be made by anyone, and because this examination was not deemed of importance in the present work.

Color Vision of the Monkey. It has been said above that the animals soon learned to discriminate the colored breads, leaving the bitter ones and taking the sweet. At first this may be taken as sufficient evidence that the discrimination was made on the basis of the color quality, but to this conclusion many animal psychologists will take exception. On account of the doubt on this point a supplementary series of tests were made with several monkeys to determine whether the hue or the difference in intensity (or brightness) was the factor which was discriminated. The tests of one animal will suffice to make this matter clear.

With this animal breads colored red and green were employed. After the animal had learned to discriminate the two kinds of bread, the colors were changed in intensity or in saturation and additional tests were then made. In the original tests (before the changes in the intensity, etc., of the colors) in making up different lots of colored breads the pieces showed slight differences in intensity and in saturation, and to a greater extent in moisture. These variations were not of sufficient amount to be particularly noticeable by me or by others, and were determined only when the breads colored at different times were placed side by side and examined in very bright light. There was also a very slight difference in the colors of the individual pieces of bread, but under the most favorable conditions these differences could not be detected at a distance of about a meter. In performing the experiments the colors were placed on the plate simultaneously but in an irregular position; half of the time the red was to the right of the animal and half to the left. Sometimes the red and sometimes the green was nearer the animal. In this way no association could be formed between positions and the tastes.

Experiments in color (beyond these performed by Dr. Shepherd, to whose work reference has been made) began with monkey 6 on November 30. On that day in the first test the animal took both pieces of bread, red and green, and immediately placed them in her mouth and ate them. She did not appear to enjoy the

taste of one of the pieces and became more cautious after two trials, and on the third trial she partly rejected the green (bitter). On seven other trials on this day the animal took only the red bread (sweet). On the following day she took only the red. Three days later she took both the red and the green in the first seven trials, and only the red the other three trials. A rest of two days was given, and, following this, on twelve successive days, ten trials each day, there were only three mistakes. At the end of this time it was concluded the habit was firmly established. The animal would not take the green bread, no matter what position it occupied on the plate. At times the red bread was placed as far away as possible, and at other times to the extreme right or to the extreme left while the green was in the most prominent place, viz., the center of the glass.

On the following day the conditions of the experiment were altered. Instead of presenting to the animal only the definite colors with which it had become familiar and to which it had learned to react in a definite way, marked variations of these colors were presented. Each of the stock solutions for this experiment was mixed with different quantities of water, and in this way there was obtained solutions giving (to man) four distinct shades of red and four of green. The mixtures which gave the different colors and the color results on the bread (according to the Milton-Bradley scale) of saturating the bread with these mixtures are as follows:

Red 1—(red, shade no. 2) 16 cc. standard red solution, 40 cc. water, and a piece of sodium carbonate the size of the head of a pin.²¹

Red 2—(red) 16 cc. standard red solution, 40 cc. water, and sodium carbonate until the solution began to clear.

Red 3—(orange red) 16 cc. standard red solution, 40 cc. water, and sodium carbonate until the solution became clear.

Red 4—(red, tint no. 2) 10 cc. standard red solution, 50 cc. water, and sodium carbonate until the solution became clear.

Green 1—(yellow green, shade no. 2) 20 cc. standard green solution and 50 cc. water.

²¹ The Congo red is an indicator for acids and alkalies, and the color of the solution depends upon the relative amounts of these opposing conditions. The color of the solution was readily altered by the addition of small quantities of sodium carbonate and the greater or less quantity of this alkali gave the various shades and tints of the red.

Green 2—(green, shade no. 2) 35 cc. standard green solution, and 20 cc. water.

Green 3—(green, tint no. 1) 15 cc. standard green solution, and 25 cc. water.

Green 4—(yellow green, tint no. 1) 10 cc. standard green solution and 50 cc. water.

Two of these colored pieces of bread were presented to the monkey in the same manner as the breads to which it had learned to react. The breads were selected and arranged in an irregular order; red 1 and green 1; red 1 and green 2; red 2 and green 1; red 3 and green 3, etc. With these combinations twenty tests were made and in every test the animal took the red bread and disregarded the green. Twenty tests on each of two succeeding days gave the same result; green of any shade or tint was not taken, the red bread of any shade or tint was taken. Three days later twenty-two tests with two of the colors gave almost the same result, and two tests in which two pieces of green and two pieces of red were presented simultaneously showed that the animal would take the red and disregard the green even when the conditions of the test were changed in this particular. Following is a record for this day:

December 23, 1909. Monkey 6, reactions to shades and tints of red and green. R₁, R₂, R₃, R₄, G₁, G₂, G₃, G₄, are the different shades and tints used. The color noted first in each test was placed at the left of the plate.

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| 1. R ₁ , G ₁ . | 9. G ₄ , R ₁ . | 17. G ₃ , R ₃ . |
| 2. R ₂ , G ₂ . | 10. G ₄ , R ₂ . | 18. R ₄ , G ₃ . |
| 3. R ₃ , G ₃ . | 11. G ₂ , R ₁ . | 19. G ₄ , R ₁ . |
| 4. R ₄ , G ₄ . | 12. G ₂ , R ₂ . | 20. R ₂ , G ₄ . |
| 5. R ₁ , G ₄ . | 13. R ₃ , G ₂ . | 21. R ₃ , G ₄ . |
| 6. G ₄ , R ₄ . | 14. R ₄ , G ₂ . | 22. R ₄ , G ₄ . |
| 7. R ₂ , G ₄ . | 15. R ₁ , G ₃ . | 23. R ₁ , G ₁ , G ₄ , R ₄ . |
| 8. G ₄ , R ₃ . | 16. G ₃ , R ₂ . | 24. R ₂ , G ₂ , R ₃ , G ₃ . |

In test 4 the monkey took the red piece first and after it the green; in all other tests only red was taken. Eighteen days later the monkey made no mistakes in 13 trials with two pieces presented simultaneously. Following is the order of the tests:

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|--------------------------------------|---------------------------------------|---------------------------------------|
| 1. R ₁ , G ₂ . | 6. G ₄ , R ₃ . | 11. G ₁ , R ₄ . |
| 2. G ₂ , R ₃ . | 7. G ₄ , R ₂ . | 12. R ₂ , G ₁ . |
| 3. G ₂ , R ₂ . | 8. R ₄ , G ₄ . | 13. R ₄ , G ₃ . |
| 4. R ₄ , G ₂ . | 9. R ₁ , G ₁ . | |
| 5. R ₁ , G ₄ . | 10. G ₁ , R ₃ . | |

Another monkey which was tested picked out the shades and tints of red when the number presented simultaneously was more than four, and two other animals made no mistakes on the different shades at a later date although the colors were not so defined as they were in these experiments. Even when other colored solutions were used for the dipping of the bread the animal always selected the red in preference to the green. In some of the later tests the red bread was made by dipping the pieces into Carter's crimson ink, and the green by dipping pieces into a mixture of blue and yellow drawing inks (Columbia brand). These later tests were made also without having the solution mixed with thymol solution, and without the addition of saccharine and quinine. It appears from these tests, therefore, that neither the smell of the thymol nor the presence of the saccharine affected the reaction after the habit was established and that the reaction was one due to the color of the breads.

In further tests with monkey 6 in which some of the colored pieces of bread were of a larger size, 10 to 12 mm. on each edge, and some were of the smaller usual size, the combinations of the small with the large did not affect the reaction to the color. The following combinations were made: A, small red with a large green; B, small red with small green; C, large red with large green; D, large red with small green; tints and shades varying as has been described. In these tests the red was selected regardless of size, tint or shade; the green was always disregarded.

The results of these experiments indicate plainly that the monkey which has been taught to react to a particular situation is able in some way to transfer this practice to a situation, which has certain elements of similarity, or, to put the matter in more definite terms, the monkey which has learned to react to definite colored objects in some manner extends this mode of reaction to shades and tints of the same hues. Two explanations may be offered for this result; that the animal has in each case reacted to the hue of the object or that it has reacted to some other factor. The experiments with the different sizes shows that the reaction was not to size. The fact that the colored breads were placed on the plate in irregular order indicated that the reaction was not due to the position. Because the animal disregarded the differ-

ence in intensity of the colors and continued to react in an appropriate manner regardless of the intensity differences is an indication that the intensity relations of the breads was not the important factor, and because other reds and greens (with presumably different absorption spectra, although this was not determined) were selected by the monkey in the same manner, the indication appears plain that the reaction was due to the color or hue. From the tests, especially when we compare them with other tests on animals performed by others, it seems justified to conclude that monkeys do discriminate colors, and that in the present work the discrimination was one of color and not of other elements in the stimuli.

Operation. Each monkey was caught in a bag and anesthetized in it before being prepared for the operation. This was found to be necessary on account of the struggles of the animal from the anesthetic and binding, and after the anesthetic was begun it was continued throughout the time the operation was in progress. As an anesthetic I used the A. C. E. mixture. This I found was well taken by the animals, it decreased the time of excitement, and was much safer than when chloroform alone was given. The recovery was rapid and attended by no adverse conditions.

After having been anesthetized the animal was placed upon an operating board, and tied thereto. The hair over the head and over the back of the neck was cut closely, and over the part where the incision was to be made a depilatory solution or paste was applied. This was found, as in my previous work, to be an excellent method of removing the hair, and was much easier than the method of shaving. In shaving it has been found that the skin is sometimes broken and this introduces an extra wound which must be attended to or the chance of infection is increased. Moreover the depilatory produced a "close shave" removing the fine hairs as well as the coarse ones. The mixture employed by me was made by combining equal parts of precipitated chalk and barium sulphide, and adding sufficient sterile water to make a paste.¹² This was applied to the head, as has been mentioned and

¹² I tried upon myself various mixtures, but found the most satisfactory results to be obtained with the mixture of chalk and sodium sulphide, calcium sulphide, and barium sulphide. The calcium and sodium sulphides did not work as rapidly as the barium, and were apparently more irritating, so that in all the experiments with the monkeys the barium sulphide was employed.

in from three to five minutes the hair was removed by scraping away the paste. The head was then washed with bichloride solution, 1 : 1000, and a gauze sponge soaked in the solution of bichloride was applied to the head, and left there until the operation was begun, i. e., about 3 to 5 minutes.

The instruments had been sterilized by boiling in a solution of sodium carbonate, and the dressings had been sterilized in a pressure steam sterilizer. After having carefully attended to the matter of sterilization, the operation was begun. The apparently great trouble taken for the production of asepsis was repaid by the results of the operations, in only one of which were signs of infection found, and this case was readily explained and will be mentioned below.

A median incision was made in the scalp and a second one was made at right angles to the original one, about at the place the skull openings were to be made. The skull was opened by the use of a half-inch trephine, and in most of the operations this opening was enlarged by bone forceps to three or four times the original size. In some of the operations the brain was incised with a fine cataract knife, for the purpose of separating the occipital lobe from the remainder of the brain, but in most of the operations the cortex was cauterized by means of a galvano-cautery. The depth of the burning varied in each case, in each hemisphere and even in different parts of the hemisphere, but these results will be mentioned in the appropriate experimental sections. The trephine buttons were seldom inserted. Bleeding from the diploe was checked by the application of bone wax, and the bleeding of the brain was usually controlled by the application of compresses wet with normal saline solution. At times it was necessary to apply a small quantity of Suprarenalin, 1 : 1000, which effectively checked the hemorrhage when it was used.

After the operative interference with the brain the dura was drawn over the brain, but was not sutured. The scalp wound was closed, and over the site of the scalp wound a layer of gauze slightly thicker over the site of the removal of the bone, was placed. A cotton bandage was then arranged around the head, to include the jaw, but leaving out the ears. After sufficient layers of this had been applied it was sewn with strong thread to

prevent its removal by the animal. The bandage was picked at by the animal but in no case in which the sewing was thoroughly performed did the monkey cause it to fall off. The constant picking frayed the edges and after a few weeks the bandage entirely disappeared from the head, and was found encircling the neck like a ruching. Long before this time, however, the wound had healed and the removal of the bandage did not affect the wound. After the bandage was removed it was usually found that the hair had begun to grow. With one animal the bandage was hurriedly sewed, on account of the rapid recovery from the anesthetic, and the whole bandage was found to have been removed within three hours after the operation. Even this animal did not show signs of infection at the autopsy. In this case a new bandage was applied after the head was thoroughly washed with bichloride solution.

Post Mortem Examinations. One animal was killed with illuminating gas, and the others were given overdoses of chloroform. In most cases the autopsy was performed immediately after death, and in only one was there a delay of as much as two hours. The brains were weighed as a whole, and no effort was made to separate the cerebrum from the cerebellum and pons, etc., nor to get the separate weights of the hemispheres. It was found inconvenient at times to get the brains weighed at the time of the autopsy, and those which were not weighed at the time of the autopsy were weighed after having been preserved in alcohol (95%) or in formalin (10%) for 24 hours.

Some of the brains were photographed immediately after the autopsy, but a few were photographed after having been in alcohol or in formalin. From the photographs tracings were made, showing all the principal points, fissures, etc., and the illustrations accompanying this article are reproductions of these tracings. Whenever a brain was photographed it was placed on a scale showing centimeter and half centimeter divisions, and these divisions are marked in the illustrations. The surface diagrams are, therefore, approximately accurate representations, and on account of the presence of the scale the accurate sizes of the brains may be determined. The illustrations are reductions from the tracings, in some cases to one-half size, in others to two-thirds

size of the originals. When the hemispheres were cut for the purpose of rapid hardening and impregnation, sketches were made of the appearance of each section, with the apparently abnormal parts distinctly marked.

After complete hardening, sectioning, staining and mounting of the sections they were placed in the Edinger projection and drawing apparatus and accurate sketches made of all appearances. The magnification was usually about three to four diameters. On each of these drawings the scale was drawn. Reductions of these sketches are reproduced.

The sections were examined microscopically by Dr. G. R. Lafora, and the results of his examination are noted on the diagrams. Wherever the cortex or pia or fibers were diseased marks were made on the drawings (Edinger) and at the same time Dr. Lafora marked the limits of the so-called visual (i. e., calcarine type) cortex if these were determinable. At times it was found that all distinctive marks of the calcarine cortex had disappeared from the lateral aspect of the hemispheres, and at times it was even difficult to note the limits along the calcarine fissure. In estimating the amount of destruction and in drawing this on the diagrams it was done conservatively, but on the other hand, on account of the negative character of some of the results, there was a liberality in defining the limits of the calcarine cortex. It is possible that other observers would have judged these two limits to be closer together than they are pictured in the accompanying illustrations.

Following is a summary of the histopathological findings by Dr. Lafora: "The lesions of the pia may be summarized as follows: In places where the galvano-cautery acted directly upon the pia (i. e., burned it) the pia was found to be completely destroyed. In certain cases and in certain places the following effects of partial destruction were found: hemorrhagic processes with an accumulation of polymorphonuclear leucocytes and Körnchenzellen (full of pigment and fat) and a hyperplasia of the fibroblasts. These are reactive processes consequent to a partial destruction or to an irritation of the pia a short time previous to the examination. A similar condition, though not so marked, is found in the pia over a comparatively large area sur-

rounding the cauterized regions, but the pial alterations in the latter cases are not accompanied by cortical changes as are those in the cauterized area. In the cerebral cortex numerous pathological changes, especially different forms of degeneration, have been found. When the pia was totally destroyed the cortex was found to have disappeared, and to be replaced by an accumulation of blood cells with polymorphonuclear leucocytes, around which there were many *Körnchenzellen* and macrophages. Around the necrotic zone, which sometimes extended to a considerable depth into the white substance, was another zone of a reactive character. In this secondary zone there were found marked hyperplasia of the blood vessels, many fibroblasts showing a transformation into macrophages, and hyperplasia of the glia cells. The nuclei of the glia cells frequently showed karyorrhectic processes, and these cells were charged with pigment, and apparently took the functions of the *Körnchenzellen*. When the pia was only partially affected there was produced a marked reactive process, in which condition we found very different lesions. At times it was found that the zone *agranularis* was destroyed and the other cortical layers were only slightly affected by a neuroglia hyperplasia and many ganglion cells showed tigrolysis and neurophagia. At other times the process was more marked in the middle layers of the cortex (*granularis interna* and *ganglionaris* of Brodmann) while the *zona agranularis* (*lamina zonalis* of Brodmann) and the *lamina pyramidalis* were not much affected. These conditions are undoubtedly dependent upon the different vascular destructions. In the last described conditions in the vicinity of the cortex in which the lesion was pronounced there were always found indications of irritative changes (neurophagia, neuroglia hypertrophy and hyperplasia), the vessels showed many *Körnchenzellen* in the lymphatic spaces and there were also observed neuroglial *Stäbchenzellen*. We observed hemorrhagic, necrotic and reactive processes. Where hemorrhages were produced a reaction around these areas was always found. Sometimes the obstruction of a vessel produced a necrotic process in the depth of the white substance, with a preservation of the cortex. In the brains of the animal operated upon by cutting, the degenerated elements (fat formation) were not well marked on account of

the short time intervening between the operation and the time of the examination. In these cases, however, some fat spherules were found, as the drawings indicate."¹³

It may be said that the examinations of the brains were made after staining by the methods of Nissl and Marchi.

One point mentioned in Dr. Lafora's account of the lesions is worthy of special attention, viz., the fact that in certain places it was found that destruction of the pia was not accompanied by destruction of all of the underlying cortex. In selecting the cautery method of extirpation it was believed that the burning of the cortex with the consequent obliteration of the vessels would produce an anemia of the cortex, and a consequent destruction. By the use of injection methods Beevor (4) concluded that the cortex is supplied by the arteries passing through from the pia.¹⁴ The results of the histological examinations in these cases appear to indicate that not all parts of the cortex are supplied by the pial vessels, and that some parts of the underlying white substance may be supplied. Some of the more interesting, but finer observations on this point have not been mentioned in the account of Dr. Lafora, but it appears true that contrary to Beevor's view the cortex is not entirely supplied by the pial arteries. In some cases, as has been mentioned in the above account, there has been a selection even in the different layers of the cortex, and it is neither true that all the cortex is destroyed nor that it may be preserved entirely.

¹³In all the figures (except 93 to 96) indicating the conditions found by microscopical examination, dots are made to indicate the location of hemorrhagic processes and crosses to indicate the destructions of pia, cortex, fibers, etc. In figs. 93 to 96, the degeneration of masses of fibers could not be well shown by crosses and the degenerated bundles are shown by dots and lines. The following abbreviations are used in the figures: *R*, right; *L*, left; *calc.* or *calc. fiss.*, calcarine fissure; *cent.*, central or Rolandic fissure; *Sylv.*, Sylvian fissure; *par.*, parallel fissure; *occ.*, occipital fissure; *thal.*, thalamus; *lent. nucl.*, lenticular nucleus; *vent.*, ventricle; *posterior* (in figs. 30-35) indicates the posterior portions of the sections; *c* indicates the limits of the calcarine type of cortex.

¹⁴"I hold with Duret that the arteries which penetrate and supply the cortex are end arteries, and do not anastomose with their contiguous branches; and I have found that, if the pia matter be carefully removed, or a circular cut be made in it, the subadjacent cortex is not injected by the vessels in the surrounding cortex."

RESULTS

Eight monkeys were used as the subjects in this work. These, as has been said, had been previously used in an investigation on animal intelligence by Dr. W. T. Shepherd, and in the early work they were given numerical designations instead of names. These numbers were adhered to in the present work, although they do not conform with the order in which the observations or the operations were made. The following accounts of the experiments are, however, given in accordance with the serial numbers for the purpose of convenience.

An examination of the case records reveals a variety of operations, and a variety of observations on different aspects of monkey intelligence, some of which can, and some of which can not, be correlated with the brain conditions.

Monkey 1. This animal was trained to discriminate yellow (bitter) from red (sweet) bread. The experiments for the production of the habit were begun September 23, and continued over a period of more than two months before an operation was performed. In the training, tests were made on 29 days of the whole period. Following is an abbreviated account of the process of learning: Sept. 23, 10 tests; took yellow bread twice and the other eight trials left it, but always took the red bread. Sept. 24, 10 tests; similar result, except that the monkey did not place the yellow in its mouth the second time it picked it up. On the two succeeding days there was a similar result, although on the fourth day the animal more often picked up the yellow bread. On the two succeeding days the monkey always picked up and ate the yellow bread. On the two succeeding days the monkey always picked up and ate the yellow bread, and did this apparently from hunger, although he did not seem to like the taste. On Sept. 30, 10 tests; the animal first showed perfect discrimination, for it always took the red bread and left the yellow. Two days later a similar result was obtained, but on Oct. 4, after one day's rest, the animal once more took both the red and the yellow. From this time there was a gradual curve of learning, with only slight variations, to Oct. 19, after which time the monkey made very few mistakes. On November 3, the animal performed the ten tests satisfactorily, and a rest of fifteen days was given, at the end of

which time a "memory test" was made. This latter showed perfect retention. After five days another series was made, showing perfect retention, and again three days later, Nov. 27, with the same result.

Operation: November 27, 11 a. m. The occipital lobes were severed by a cut posterior to the parieto-occipital fissure. During the operation there was little loss of blood from the scalp, bone and brain, except on the right side where there was considerable hemorrhage when the incision was made into the brain substance. This part of the brain was compressed for about ten minutes, and at that time the blood flow ceased. The trephine buttons were inserted, the head was bandaged, and the animal was taken from the operating table at 11:30.

Immediately after the operation the eyes were widely dilated, and the animal appeared to be only semi-conscious.

Two and one-half hours later the animal was found sitting up in the cage in which it had been placed, but it was in a huddled attitude. When a lump of sugar was handed to it, it took the sugar but threw it upon the floor of the cage. It took a piece of cotton wool, which had been rolled into a swab, and threw it upon the floor of the cage. Its attention could be attracted easily when noises were made, and it started when touched and when the door of the cage was shaken. When the hand of the observer was placed near the cage netting the animal put one of its hands in that direction, and at times took hold of the finger. The movements that were made were slow and indecisive, and the errors made in putting its hands through the wire netting were from 2.5 to 5 cm., i. e., one to two meshes of the netting. When the gaze was attracted to an object held in the hand and a lump of sugar was exposed in the other hand of the observer, but peripherally to the fovea, the animal would sometimes turn to the sugar, but most, if not all the movements of attention appeared to be due to the sound stimuli rather than to visual sensations. In one visual respect the animal was quite unlike a normal animal; it kept its gaze fixed on one point for comparatively long periods, and the quick shifting gaze of the normal animal was entirely lacking. When no sound was made, and movements of the observer were at a minimum the animal remained absolutely quiet,

the eyes became staring, the eyelids gradually lowered, until the eyes were covered almost completely. Slight noises or movements (which very likely produced noises although they could not be seen) waked the animal immediately, and there was a return to the sleepy condition immediately when quiet again reigned. The pupils were medium in size.

Thirteen hours after the operation, the animal was looked at; when the light was turned on the animal came from behind the door of its cage, looked at the observer, and appeared to be bright. No special observations were made at this time.

Thirty-two hours after the operation, the animal was with some difficulty removed from its sleeping box to the observation cage. Sticks introduced into the sleeping box to persuade it to change its quarters were taken hold of and twisted away from its body. When it went into the observation box, it was presented with a lump of sugar, which it took with a rather awkward movement, and immediately placed in its mouth. Several times it took the sugar from its mouth, held it in its hand and looked at it, and returned it to its mouth again. When the end of a stick or the finger was put a short distance through the wire netting of the cage the animal made movements to take hold of these things. The movements were not the quick lightning-like movements that a normal monkey makes, but slowly executed and inaccurate. The arm moved toward the goal, or rather, in the direction of the goal, but when the hand of the animal was about 5 to 8 cm. away from the object an additional adjustment was made so that the finger or the stick could be grasped. It was apparent that the initial adjustment was very inaccurate, that the distance was not properly sensed, but that the discrepancy was appreciated, and that the secondary or later adjustment was considered necessary. The actions of the animal during these tests could be compared best to those of a child or even of an adult who tries to make a new or unaccustomed movement, of a rather complex character, and who attends to the movement with the eyes as a guide.

The indefiniteness or impairment of the movement may have been due to one or more of several factors: (1) the loss of rather definite visual sensations which are normally present and

important in the adjustment of movements; (2) to a motor disturbance; or (3) to a loss of kinesthetic sensations from the eye. That there was no motor disturbance was apparent from the movements of a reflex character made by the animal. The bandage was apparently disagreeable, and caused some irritation. At times the animal put its hand and plucked at the bandage near the ear, pulled at it and endeavored to get rid of the irritation. These movements were performed accurately, quickly, and without the second adjustment which was noted in the case of the movements of taking hold of the finger or of the stick.

The following day the animal gradually improved, and it was impossible for me to determine any visual defect. The movements continued to be a trifle uncertain, but this was all that was observed to be different from a normal monkey.

Two days after the operation the animal was tested with the colored breads. In these tests, the animal made no mistakes, although at first the animal was not as quick as normal, and it was usually necessary to wait a longer time for the animal to select the bread. In the tests there was no memory or association loss, and, as far as could be determined, the animal reacted to the colors just as it had done previous to the operation. After an interval of a day the animal was again tested and found to retain the ability to discriminate the colored breads, and from this time there was no appreciable disturbance in the behavior of the animal. This animal could, therefore, see, and there was neither a visual disturbance similar to the permanent blindness noted by many observers, nor to the temporary blindness recorded by Ferrier. Objects in all parts of the field were accurately seized and apparently discriminated, and no defect of the nature of restriction of the field of vision or of a segmental blindness was found.

Four months later a second operation was performed. In the intervening period the animal was not practiced much on the color discrimination, but sufficient tests were made with him so that this habit was not forgotten. No other visual discriminations were taught to the animal. During part of this time the discrimination tests on the red and yellow showed that

at times the animal took occasionally the yellow piece of bread, smelled it, or tasted it and sometimes even ate it. In a series of ten tests this was found to occur but once, and to be as often in the middle of a series as at the beginning. The animal was well fed during this period, and at times refused to work with the colored breads, so that only three to five tests were made on certain days. Following are accounts of the color tests immediately previous to the second operation:

March 29, 10 tests: in the second test the monkey took the yellow and the red at one time and placed both in the mouth and ate them; the tenth test the animal did not take either of the pieces, although I waited five minutes; in the other eight tests the animal took only the red bread and this was taken immediately and without hesitation. March 30: the first seven tests were made in the usual manner, the animal taking the red bread as soon as the breads were presented; in the eighth test the animal took the yellow with the left hand (in all other tests the animal had been taking the bread with the right hand) and tasted it, but threw it away without eating; the results of the ninth test were correct, but the monkey did not take the red bread on the tenth test until after thirty seconds. March 31, 5 tests: in the first test the animal took the yellow bread as well as the red, but threw it away after having tasted it; the other four tests were correct. April 1: on the first test the animal took the red bread and left the yellow, but did not take either piece on the second test and the tests had to be discontinued.

Second operation, April 1.—The trephine openings were made farther back in the skull so as to expose the tip of the occipitals. The buttons which had been inserted at the time of the first operation were found to be present, but softened and adherent to the dura. When they were taken away there was considerable bleeding. Both openings (including the two trephine openings on both sides) were enlarged with bone forceps. The dura was cut to expose the surface of the occipital areas. The cortex was then burned with the electric thermo-cautery at a red heat. The cautery was passed over the lateral aspect of the lobes,

and inserted into the mesial part. The operation was finished in an hour (12:30 p. m.).

An hour and a half after the end of the operation the animal was inactive and appeared to be sleepy. The eyes appeared to be turned from their normal positions, the left especially being turned inwards and upwards. When a finger was put through the wire netting the monkey crouched back into the corner of the cage. When a piece of bread was held in front of the cage the monkey put its hand awkwardly towards the bread, took it and conveyed it to the mouth. Other pieces of bread presented through the wire netting were not taken. A small piece of bread dropped on the floor of the cage was apparently seen, for it was picked up immediately and without hesitation. A piece of banana was presented through the wire netting about 30 cm. from the animal. The left hand was put forward, although the left side was farther from the food, and the banana was grasped in an awkward manner. Other pieces of food were presented and were sometimes taken and sometimes not taken. When they were taken the movements were slow and inaccurate. A short stick thrust through the netting did not cause the animal to make the usual defense movements of grasping and shoving, but the left hand was put forward to take the object. When the stick was quietly inserted into the cage in any direction the eyes of the animal were directed toward it when it came sufficiently near (about 15 cm.), and although the animal did not fight against it as a normal animal did, the eye movements were exactly like those of a normal animal. A white dish containing water was placed in the cage, and the animal soon placed its head in the dish and drank. A second attempt to drink was more quickly made. It was noted as peculiar that the monkey did not use the right hand for grasping, but used the left which performed the movements in an uncertain manner. Previous to the operation this animal usually used the right hand. At the time the notes were made the following question was inserted: How much of the inaccuracy in movement is due to the heterophoria? This question has not been answered and even at present I have no indication of the part this may have played in the motor inaccuracy. Throughout these observations the animal

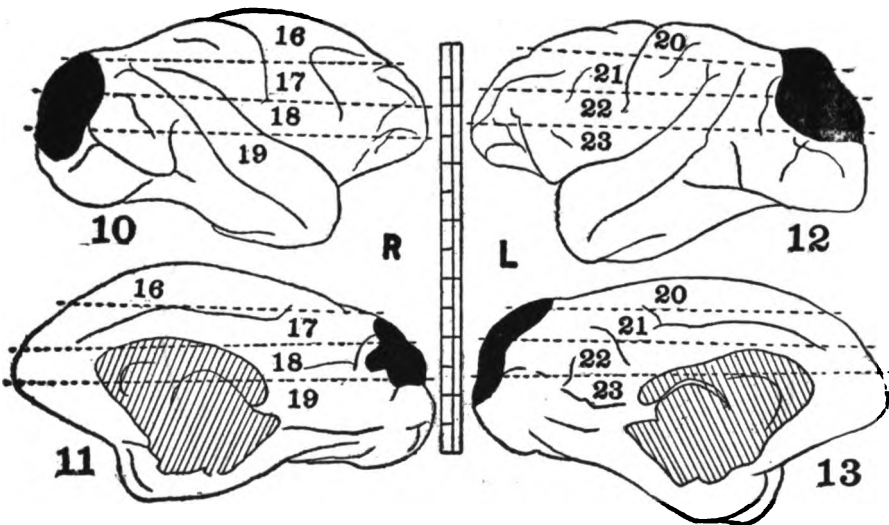
appeared to be under the influence of the anesthetic, to be sleepy, with lack of energy, little attention and a "don't care" attitude.

Twenty-seven hours after the operation the animal seemed perfectly normal, except that his movements were slow and deliberate, and quite unlike the quick sharp movements of a normal animal. Pieces of raw sweet potato were given to him; these he took slowly and put into his mouth only after having smelled them. He ate them in a normal manner. Once a piece of potato was dropped on the floor of his cage, he looked at the food, then at me and reached for the food which he obtained although all the time he kept looking at me. Apparently, the animal had made a judgment of the location of the food, and acted upon this judgment even when the eyes were turned from the food. He drank water from a dish apparently without difficulty, and picked up particles of food from the floor of the cage. When a stick was presented to him he slowly reached out his hand and took hold of it. His attention was attracted by someone in front of the cage and a stick was placed in the back of the cage, care being taken to make no noise, and when the stick came within the field of vision he immediately turned and often seized it. This reaction was the same whether the stick was to the right or to the left. The angle of the stimulus was not accurately determinable, but it appeared that the animal could see objects in the outer portion of each field, i. e., with the nasal portions of the retinae, at least as far away as 70 to 80 degrees. The angle of vision above the head was apparently normal. There was a decided preference for the use of the left hand, but it did not appear that the right hand was in any way paralyzed, although from his continued use of the left it would appear that there was a paresis or some similar change from the normal.

Three days after the operation the animal appeared the same as that just noted. On this day, five tests with the colored breads were made. On the first, second and fifth the monkey took only the red bread, but on the third and fourth tests took both yellow and red. Two days later, five tests were correct. The animal was exhibited before the Georgetown Clinical Society on April 4, and at that meeting the members were unable

to note any difference from the normal. From the results of the bread tests it was apparent that the animal retained the ability to discriminate colors, and although the tests on the third day after the operation were not entirely successful, they were quite as successful as they had been immediately previous to the operation.

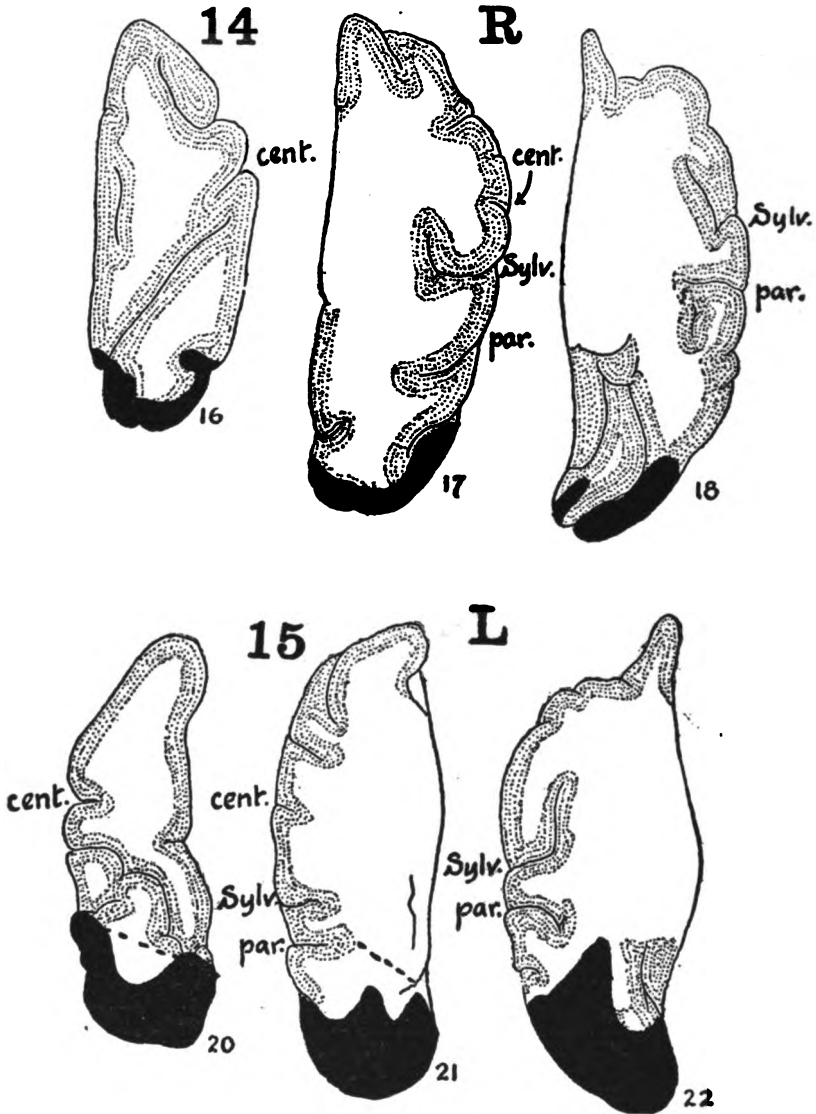
Nine days after the operation (April 10), it was noted that the animal was not inclined to work. It did not eat. During the night of the 9th the animal tore the bandage from the head, and opened the scalp wound slightly. On the following day the scalp was swollen, and the brain appeared to be bulging through the trephine openings. The animal did not work on the tests, and, as it refused to eat, no observations of vision or of movement were possible. At 8 p. m. of this day the animal was found to be quite stupid and dull. It took a long time and a violent stimulus to arouse him. At one time the eyes were rolled upwards and showed a slight nystagmic movement. At another time there were twitchings of the hand. The animal



FIGS. 10, 11, 12 and 13. Monkey 1. Lateral and mesial aspects of the cerebral hemispheres, showing the parts destroyed by the operation. The latter is a reconstruction. Slightly reduced (cf. scale). Traced from photograph. The small size numerals are the sections from which other figures (14-23) were drawn.

was killed with chloroform at 9:30, and the autopsy was performed immediately.

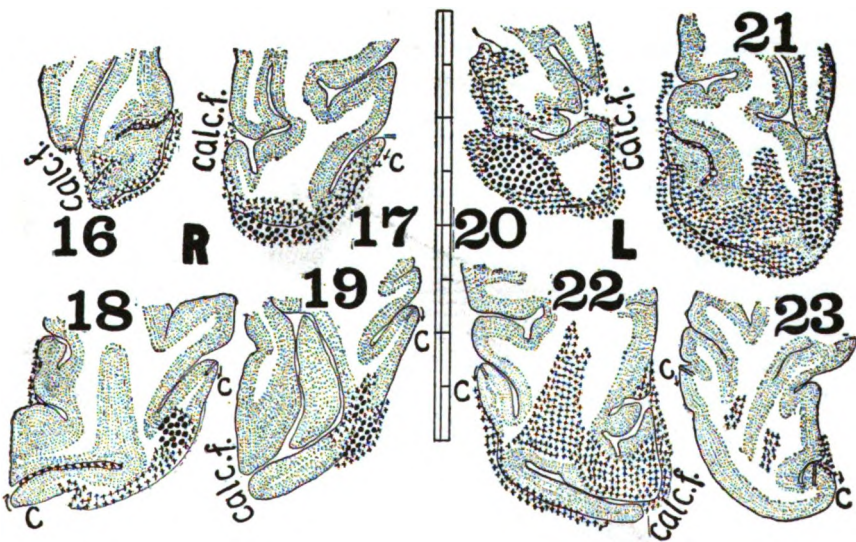
At the autopsy the scalp in the neighborhood of the operative



FIGS. 14 and 15. Monkey 1. Gross sections of the hemispheres, showing the parts affected by the operations. About natural size. Drawn at time of autopsy, free hand. The illustrations are taken from the lower surfaces of the sections indicated by lines in figs. 10-13.

cuts was found to be swollen, the cellular tissue was filled with a watery fluid which could be squeezed from it. The scalp at this place was about a half centimeter thick. The brain bulged through the trephine openings. The dura was adherent to the brain and the scalp was adherent to the dura. The brain was removed from the skull with difficulty. The posterior surfaces of the hemispheres were much reddened and soft to the touch; there was no appearance of pus. The brain was placed in toto in 95 per cent alcohol. The photographs were made thirty-six hours after the autopsy.

Figures 10 to 23 show the brain with the effects of the two operations. In figs. 10, 11, 12 and 13 are given diagrams of the lateral and mesial aspects of the hemispheres, with the parts of the cortex destroyed at the time of the second opera-



FIGS. 16, 17, 18, 19, 20, 21, 22 and 23. Monkey 1. Horizontal sections through the occipital parts of the cerebral hemispheres, showing effects on cortex, and underlying fibers of the second operation. About $3/2$ natural size. Edinger drawing apparatus.

tion. Figs. 14 and 15 show the gross appearance of the sections made for the purpose of microscopical study, with the parts marked thereon where the cortex had been destroyed. The cor-

tical effects of the first operation are not indicated, and it was impossible to differentiate these from the results of the second operation. Figs. 16, 17, 18, 19, 20, 21, 22 and 23 give the appearance of the microscopical sections, which were stained by the Nissl method, and which show the hemorrhagic and cortical effects of the second operation. It will be noted that the sections numbered in figures 10 to 15 correspond with the sections marked with the same numbers in figures 16 to 23. The areas of cortical destruction (marked black) in figs. 10-15 have been drawn from the examinations of the microscopical sections.

The results of the examinations of the microscopical sections are given by Dr. Lafora as follows: "Monkey 1, cauterization of the brain, Nissl method, horizontal sections. On the right side the occipital lobe lateral aspect is almost entirely destroyed, the lesion involving the whole depth of this part of the cortex. A part of the periphery of the calcarine type of cortex escaped destruction, and the calcarine fissure is affected only in its posterior ramification, especially the superior part. On the left side the lesion is more extensive than on the right, the calcarine type of cortex being more affected especially in the superior part of the occipital pole. The vessel lesions have produced softenings in the white matter which interfered with the fibers coming to the visual cortex. The anterior part of the calcarine fissure, and some parts of the posterior are well preserved."

Monkey 2. This animal was trained to discriminate the four colors. Red and blue were sweet and green and yellow were bitter. The training experiments began October 20. There was a gradual acquisition of the habit of taking the red and blue and of leaving the green and yellow, which became perfected (after this date there were no wrong reactions as long as the animal was in a normal condition) after six days. The tests were continued, however, a month longer, with intervals in which the memory was tested. The final tests were made on December 1, the day of the operation, and on this day the animal made no mistakes in ten trials.

Operation, December 1. The usual preliminary parts of the operation were performed in the manner described. On account

of the fact that previous sections of the brains of other animals had not been followed by visual disturbances of sufficient amount to be noticeable, if they were present, I made the incision in the brain much anterior to the so-called visual cortex, so that as much of the occipital pole as possible should be included in the effects of the operation. It was intended to separate completely the occipital lobes from the frontal parts of the hemispheres, but on account of the depth of the brain at the point the knife was inserted, all was not cut away, as was later learned at the time of the autopsy and at the time of the microscopical examination.

About fifteen minutes after the operation was completed the animal staggered drunkenly against the sides of the cage in which he had been placed. When a sudden noise was made he did not jump. The pupils were widely dilated, and there were slight tremors of the eyelids.

Twenty-four hours after the operation, the animal was found to be fairly active, engaged chiefly in pawing at the floor of the cage and scratching at the bandage on his head. A finger of the experimenter poked through the wire of the cage elicited at times a slapping of the floor, at other times it appeared that no attention was paid to it, activity at least not being directed toward the finger. A piece of rubber tubing hung through the top of the cage elicited no reaction until the animal was touched, when he made a slap in that direction. Water was presented in a saucer. The animal put out his paw to investigate, got it in the water, then leaned over and drank. A prune was held outside but near the wire of the cage. The monkey started to put out his hand to grasp it, but struck the wire and did not persist. Later the prune was held in this position again and the paw was put out and grasped the prune accurately. A prune was thrown upon the floor; three grabs were made in the general direction of the prune and each time the monkey missed it by about 7 cm. It desisted. On the following day he groped about the cage, especially on the floor when any noise was made, and did not reach for objects held in front of him, but gave some reactions to things that were held above the eyes. It seemed at times that he saw things held in the upper part of the visual

field. This was not clear, however, for at times he would not pay any attention to the food. When a stick was held close to him, he paid no attention to it as he had previously done, and there were no defense movements to push away the stick. Only when the food was placed near the nose or mouth did he make definite attempts to grasp it, and at times it appeared that his reactions were due more to smell stimuli than to visual ones.

Three days after the operation he was placed in the large observation cage into which he went from the small living box, almost as soon as the door of the latter was opened. A peanut held at the netting of the cage, about 25 cm. away from his head was well grasped and the movement was quickly executed. No hesitation was observed at this time. A dish of water was placed on the floor of the cage about 54 cm. from where he was sitting; the animal immediately went to it, drank from it in an apparently normal manner, and went back to his old sitting position. Other food was placed outside of the cage but within reach of his extended paw, and these pieces he took and ate. Small pieces of food on the floor of the cage were taken in an apparently normal manner, and the sawdust on the floor of the cage was picked over for particles of food. A stick inserted in the cage was promptly seized, and warded off. The animal moved its eyes in an apparently normal manner, and the eyes followed a moving person. The animal turned its eyes toward anyone entering the room. It moved from the large cage to the small one and reversely without mistakes regarding doors, etc., and in this respect also appeared to be able to differentiate visual stimuli.

Four hours later the animal seemed visually like a normal animal. It took food, drank water, and put its hand through the cage to get materials outside. At times its movements were not as accurately coördinated as in a normal animal but the errors of movement were not large and on this point it was impossible to make any accurate judgment. The animal scratched the bandage almost continually and it was thought that perhaps there was some pus formation at the wound. For this reason the animal was lightly anesthetized, the old bandage taken off, and a new one adjusted. The wound seemed to be in a perfectly

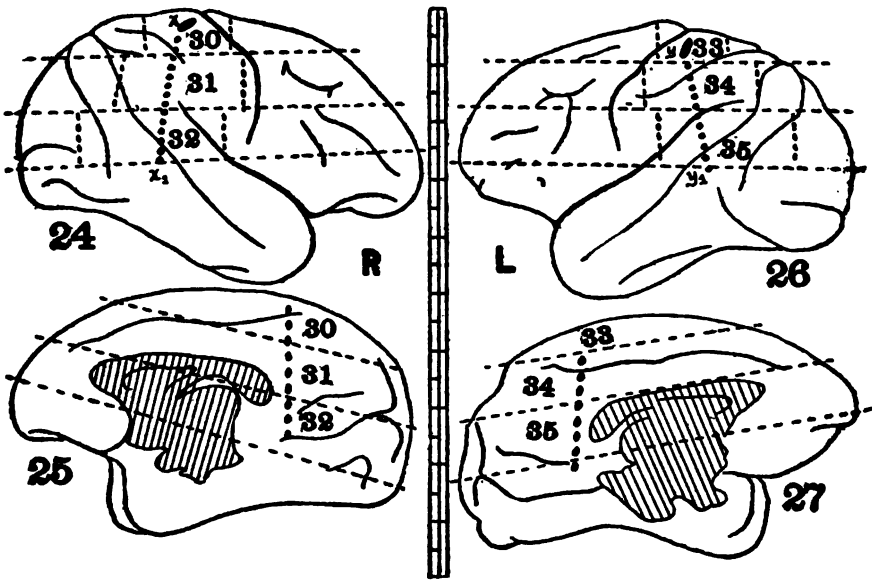
healthy condition, and to be uniting by first intention. A wet compress of 2 per cent carbolic acid was placed on the head over the wound. The animal recovered from the anesthetic in about an hour. The animal moved from the small cage to the burlap bag without any difficulty and apparently knew what it was doing.

On this day and on the succeeding days, the animal appeared to have perfect vision and to be able to coördinate the visual impressions with the proper movements. At no time after the first days was I able to determine any defect of vision, and this was also shown in the tests with the colored breads. Two days after the operation the animal made some mistakes with the colors. In all of the ten tests made on that day the animal took the yellow bread and smelled of it. Once a piece of green bread was taken in the same grab with a red, and this was apparently due to the fact that they were very close to each other on the plate. On the following day, the monkey made no mistakes in the color tests, and it seemed most likely that the mistakes made on the previous day were not due to a lack of discrimination. It is particularly worthy of notice that in one of the tests the piece of blue bread fell from his hand and that he immediately picked it up, rather than take one of the other (yellow and green) pieces remaining on the plate. The observations, and also the tests, indicated that if there had been any visual defect, it was of a nature similar to those observed by Ferrier. The general reactions of the animal were, however, so abnormal for a day following the operation, that I think we are not justified in concluding that the defect is visual and is of any special character, but I believe we should conclude that the disturbance was a general one, and is to be compared with the shock effects of operations such as are exhibited by man, especially after operations on the nervous system.

The animal was killed with chloroform, December 5. The autopsy was performed immediately after death. The brain appeared to be normal except at the points where the knife had been inserted, and there was no oedema apparent to the eye. The brain was placed in alcohol and photographed and weighed

twenty-four hours after the autopsy. The brain weighed 85 grams.

Figures 24 to 35 show the conditions that were found post mortem. In figs. 24, 25, 26 and 27, the tracings from the photographs, there are shown the points of insertion of the knife, and the sections made for the purpose of examination by histological methods. At the time the gross sections were made, drawings were made of the appearances of the sections and these are

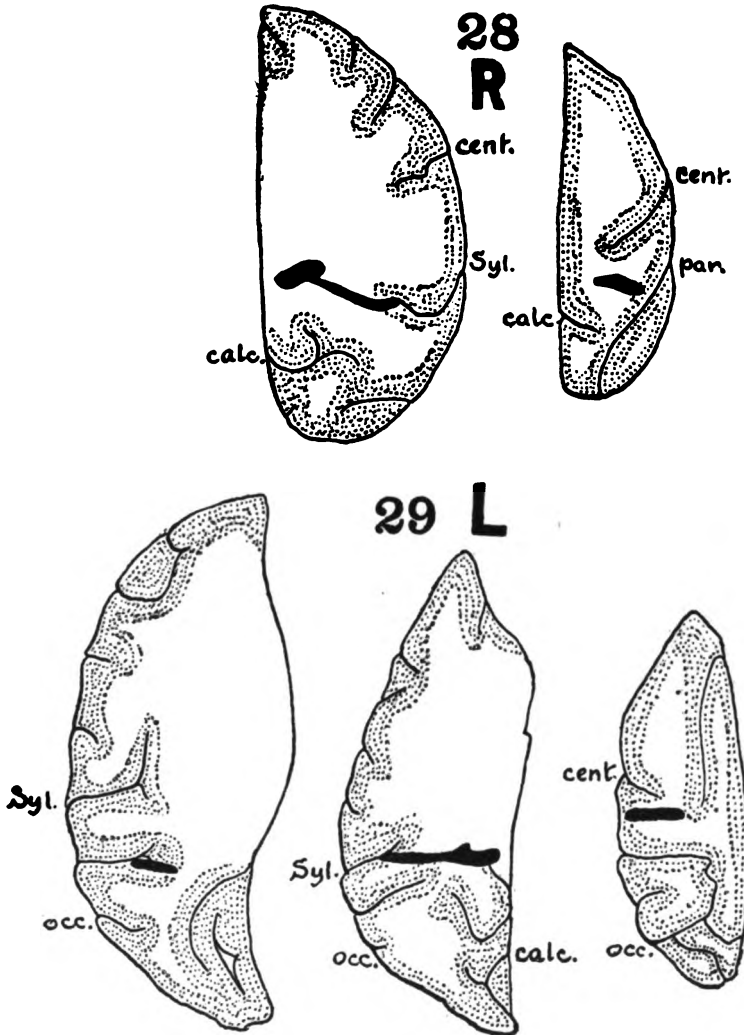


FIGS. 24, 25, 26 and 27. Monkey 2. Lateral and mesial aspects of the cerebral hemispheres, showing the course of the knife cuts into the brain $x-x_1$ and $y-y_1$ and the location of the parts of the brain examined histologically. Slightly reduced (cf. scale). Traced from photographs.

given in figs. 28 and 29. From these drawings and from a careful comparison of the brain as a whole and in the sections, a reconstruction of the course of the knife cuts has been made and these are indicated in figs. 24-27, by the heavily dotted lines. It will be seen that the knife was inserted just posterior to the central fissure and that in its course it cut away a large part of the occipitals from the anterior part of the hemispheres, but that the lower, especially the temporal, portions of the brain

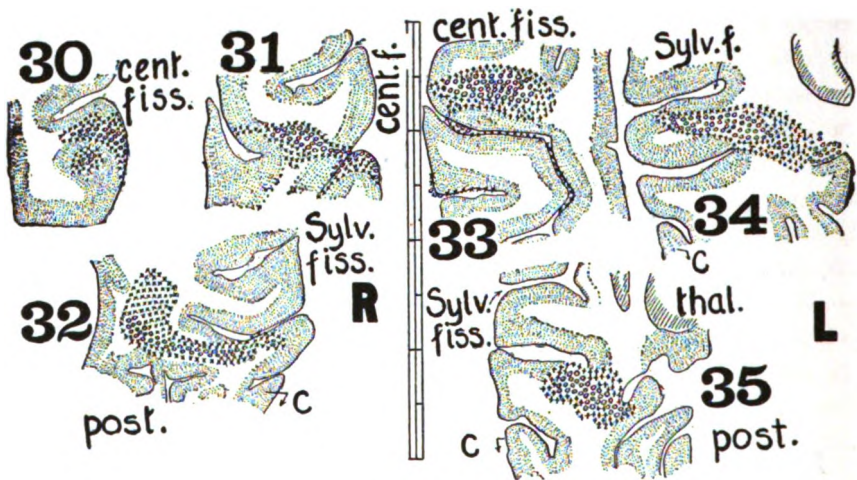
escaped. The microscopical examinations of the sections are illustrated in figs. 30-35.

The account of the histological examination by Dr. Lafora is



FIGS. 28 and 29. Monkey 2. Sections of the cerebral hemispheres showing the hemorrhages due to the sections of the brain, and the parts probably affected by the operation. About natural size. Drawn free hand at time of autopsy. The illustrations are the appearances of the lower surfaces of the sections indicated in figs. 24-27.

as follows: "The sections were prepared by the method of Nissl by Dr. Achúcarro. On both sides of the brain the sections show the lesions produced by the knife, which seem to be very extensive in its transverse directions. Although we are unable to judge of the amount of the fibers which were cut by the operation on account of the histological method used in the examination, it is sure that most of the optic fibers, except those lying low in the cerebrum, were destroyed or at least affected by the section. The cortex shows some reactive processes of the neuroglia and of the ganglion cells some of which later show a slight degree of tigrolysis. In the inferior part of the white matter the lesion is not as extensive as in the middle part. The drawings indicate the general extent of the lesions."



FIGS. 30, 31, 32, 33, 34 and 35. Monkey 2. Horizontal sections through the lesions indicated in figures 24-27. About $\frac{3}{2}$ size. Edinger drawing apparatus.

Monkey 3. This animal had been trained on the discrimination of the large and small pieces of bread. The experiments for the production of this habit were sporadically done, but the systematic training for the present paper was not undertaken until about two weeks before the operation. Following is an account of the process of learning at that time:

September 5, 10 tests: only once ate both large and small

pieces, viz., first trial; on the first, second, third and fifth trials, she took the large before the small, and on the ninth and tenth the large after the small. After the first trial, however, the animal did not eat the large piece, but smelled or tasted it and threw it away. September 6, 10 tests: took the large piece on the first, sixth and on the eighth trials, but ate the large piece only the first two times. September 9, 20 tests: took the large piece only the first two times. September 10, 5 tests: took both pieces the first three trials, but the smaller piece only on the fourth and fifth trials. September 11, 20 tests: first, took both pieces but did not eat larger; from second to twentieth only smaller. September 14, 20 tests: on the first and second trials she took only the small pieces, but from the third to the fifth the larger ones were also taken, although they were only tasted, not eaten. September 15, 10 tests: only the small were taken. September 16, 10 tests: only the small were taken. September 17, 5 tests: only the small were taken. These tests were the final ones before the operation which was begun an hour later.

Operation, September 17, 10 a. m. Occipital lobes were cauterized. There was considerable hemorrhage which was checked by hot compresses to the brain. Immediately after the completion of the operation the face of the animal was cyanotic, the eyelids closed, and the facial muscles were contracted into a grimace (*Schnauzkrampf*). The left pupil was about one and one half times larger than the right, and neither reacted to light. The head was turned backwards and towards the left; the right arm was flexed at the elbow and the left arm was extended; both legs were extended. There was hypertonia, more marked on the left.

About three minutes later the left arm became flexed at both shoulder and elbow and the left leg at the thigh; the flexion of the left arm at this time was greater than the flexion of the right arm noted immediately after the operation and which had continued. When the animal was placed on its back or on the right side there was a tendency to turn to the left, and the trunk of the animal described an arc with the concavity towards the left (*emprostotonus*). The tongue was protruded, and deviated to the left; when it was pushed into the mouth and towards

the right, it gradually returned to the protruded left position. Ten minutes after the operation was finished it was found that both sides of the animal were approximately equally flexed, and the tail was turned under the body but in the median line. The animal was tremulous; it was placed close to the fire and the tremor (shivering) ceased in about two minutes.

About half an hour after the operation the animal attempted to sit up, but immediately fell down, and remained in a rather unnatural attitude. Two hours after the operation it was found sitting up, and when I moved about the room it followed with its head and eyes all my movements; part of this may have been due to the noise made by me in walking, but the movements of the animal appeared to be rather those associated with vision.

Eight hours after the operation, the animal was sitting in its cage in a perfectly normal attitude. The contractions of the groups of muscles had entirely disappeared, and, so far as could be determined, the movements were normal in character, but rather slow. Four pieces of bread were placed approximately 5 cm. apart on the floor of the cage in the form of an arc, 30 cm. away from the eyes; for about ten seconds the animal took no notice of the food (it had not been fed on this day except at the time of making the final five tests on the discrimination of large and small) but finally accurately reached for and obtained one of the pieces in the middle; after it had eaten this piece it took the second middle piece, but during ten minutes it disregarded the other two pieces which were at the ends. Some pieces of apple were dropped into the cage; some of these the animal picked up with a great degree of accuracy, but the movements were slow and it did not immediately try to secure the food. At times the food was disregarded until some part of the animal, a hand or a foot, touched one of the pieces, whereupon she reached to the appropriate spot, secured the piece and inserted it in the mouth. A fresh fig was placed in the cage about 45 cm. distant from the eyes of the animal. This it disregarded until it was shoved near enough to the animal to be touched by it, whereupon the animal took the fig and ate half of it. After this she wandered around the cage picking up pieces here and there, but at other times her attention was attracted to

the pieces of food solely by the sight, and at times it appeared that the animal did not see but picked up the food only when it was accidentally touched by part of the body. Tests for determining the extent of the visual field were unsuccessful, for the animal did not respond to the food when it was offered on the wires. The latter observations were made at night in artificial, but good light, and it is possible that the shadows thrown by the food on the wires and the shadow of the cage on the food were distracting, although I lay no stress on this point. During all the tests mentioned in this paragraph the right eye of the animal was much inflamed, probably due to the anesthetic.

Twenty-four hours after the operation the animal was observed and tested by Dr. Lafora, who made the following notes: "The right eyelid is much inflamed, but the eye is kept open, and is used with the other; the pupils react equally to light. Objects (food) at some distance (about 60 cm.) were at first not attended to and it appeared that the animal could not see them; only when pieces were brought near its face did it try to take them. In a number of tests she continued to make movements as if to take other pieces of food in the same place although none were being presented; at other times the animal would look directly at the places and moved her head and eyes as if to follow the movements of the (imaginary) particles of food. Is this a perseveration phenomenon? Small objects, such as wires, were not well discriminated, for the animal frequently made no movement until the wires were close to, sometimes touching, the face or the facial hairs; similarly with small pieces of apple. The first experiments on the field of vision indicated that the monkey did not have a normally fine perception in the macula. for objects about 40 cm. away were peered at and the animal moved the head closer as if to get a better view before attempting to take them. All objects held towards the periphery were seen but the movements of grasping were not accurate. It is a remarkable fact, however, that the animal looked at a fly at the bottom of the cage and followed its movements with head and eyes. A piece of an apple was afterwards held on a wire in front of the animal and moved about far and near and to the right and left. The animal apparently centered its attention

upon the object which it approached and obtained after some unsuccessful attempts. After the monkey found it impossible to obtain some moving pieces of food by grasping with the hand, the animal moved the head towards the food and took it directly into the mouth. Pieces of food held to the right or to the left brought about approximately normal movements of the animal, except when the food was held at the extreme right, where it was found that movements were sometimes produced but only after a much longer time than when the food was held in any other part of the field. The hallucinatory tendency, noted above, to take objects not presented was again shown in the later movements; at times when pieces of food had not been presented for a minute or two, the animal grasped in the air or groped on the floor in the location of previously obtained food; this would indicate that the visual part of the reaction is not a perservation but an hallucinatory phenomenon. Color discrimination appeared to be preserved, at least to some degree, for the animal selected tomato three times instead of apple, its normal preference being for the former. In the later experiments the movements of grasping were accurate, showing a complete association between the movements of the hand and arm and the visuo-kinetic representation of the movement. A piece of cotton soaked in alcohol and then lighted was brought close to the animal and this experiment many times repeated showed that there was an ability on the part of the animal to see with all parts of the retina, on the right as well as on the left, the movements in all parts of the field being accurate and quick. When one piece of lighted cotton was shown and held the attention of the animal, and a second piece was brought inwards from the sides (right and left) the animal turned towards the second piece, showing that the extent of the field was approximately normal. Conclusions: Diminution of visual acuity; no diminution in extent of the visual field, at least at the sides, although it is impossible on account of the eye movements to determine whether or not there is a central scotoma; ability to discriminate colors appears to be preserved; and there is exact visuo-kinetic representation."

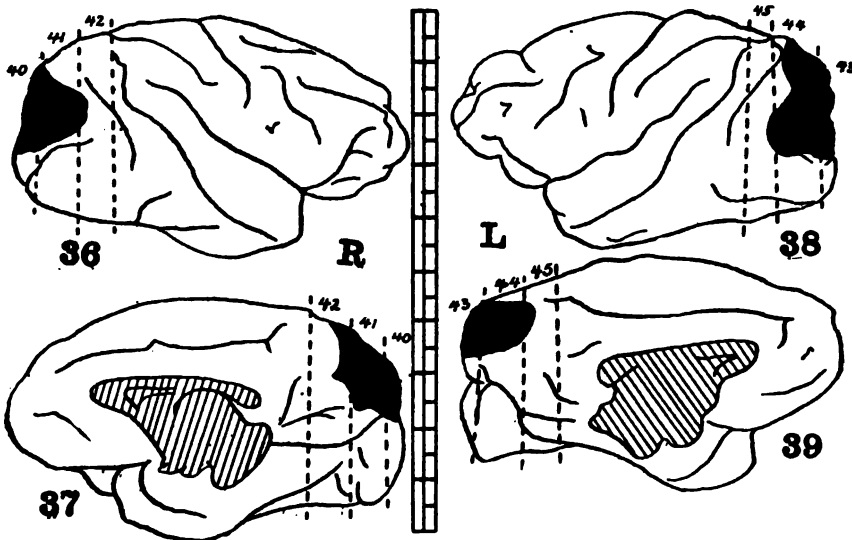
On the third day after the operation the animal was tested with large and small pieces of bread; on the first trial she selected the

small piece and disregarded the large; on the second trial she took the large piece tasted it and threw it away and then took the small piece which she ate; the third trial was a repetition of the second; on the fourth trial she took only the small piece; and on the fifth trial she got both pieces in one grasp and ate the small. The results of these experiments do not indicate plainly whether or not the habit of discrimination of the large and small pieces persisted, but the indications are that the habit remained, because each of the two times that the animal obtained the smaller piece she did not try to take the larger one which remained on the glass plate, and each of the two times that she obtained the larger piece first she returned to the plate and secured the smaller piece. In these experiments she showed the condition noted in the other tests on this day, viz., a certain amount of inaccuracy in motor adjustment. The taking of the larger piece in the fifth trial was awkwardly done, and the taking of both pieces in the second and third trials may have been due to this motor derangement. A piece of raw potato was dropped on the floor of the room close to the cage, about 75 cm. from the animal; she moved to the front of the cage, accurately reached under the bottom bar and secured the piece of food. A piece of raw apple was held on a wire about 20 cm. from the eyes of the animal, and midway between them; this she grasped on the second attempt after having made at first an error of 2 cm. After having obtained the piece of apple she grasped in the same direction (or place) although there was no food being presented, and her actions at this time gave me the same impression as they did Dr. Lafora on the preceding day, viz., that there was an hallucination or a persistence of the impression. A piece of raw potato held in front of the animal about 30 cm. away was reached for; in grasping for it the animal made a movement error of about 7 cm. at first but on the second attempt secured the food. A fly was walking over the back of the cage about 60 cm. from the monkey and to the left; the animal moved cautiously toward the part of the cage where the fly was and when near enough she grasped for it with the left hand, hitting the spot where the fly had been (the fly flying away when the hand approached). A piece of raw carrot was dropped into the cage, but fell outside; the animal groped at

the place where the carrot fell, moved to the other end of the cage, returned to its original position in about ten seconds, and picked up a piece of raw potato; then it put its hand outside the cage and picked up the carrot accurately and without hesitation. A piece of apple and a piece of potato were placed on the floor of the cage about 75 cm. away from the animal; by short steps, she moved in the direction of the food and when sufficiently near reached accurately for and obtained the potato but disregarded the apple which was on the right. Nine small pieces of food, three each of raw carrot, apple and potato, were dropped into the cage from the top, a few rolling outside and the distribution in the cage being irregular, the monkey took immediately two pieces which were within reach, then took a step and secured two pieces farther away; after having secured a fifth piece she dropped it before getting it to her mouth and it rolled away about 10 cm. from its original position but she secured it immediately; then she secured pieces of carrot and of apple which had fallen in the shadow; finally she looked outside the cage and secured two remaining pieces which had rolled outside the cage. Two small wads of cotton were dropped in the cage; for a time she disregarded these, but picked up similarly sized pieces of apple and carrot, but finally she picked up one piece of cotton and placed it in her mouth. A piece of sugar was silently placed on the floor of the cage about 70 cm. away from the animal; she stopped trying to pick up the small particles of food near her, made three steps and accurately grasped the sugar; another piece was dropped into the cage and it rolled close to the body of the animal; she peered around but did not secure it, then moved away about 30 cm., turned around, immediately reached for and secured it; a third piece was dropped in the cage in the shadow; for about a half minute this was unnoticed but then she made five steps and with an accurate arm movement secured it. Quick movements of the experimenter's hands and arms in any direction made the animal start and crouch in a corner; fingers and sticks placed through the wire netting were also startling. A piece of raw potato was placed in and stuck by its moisture to the bottom of a glass crystallizing dish, 5 cm. diam., the dish was then placed in the cage bottom side upwards; the monkey reached for the dish,

made the proper adjustment, picked up the dish, turned it in the proper position and tried to get the potato by putting the mouth in the dish; when she could not get the food in this way, she put out her tongue, dislodged the food, secured it with the right hand while the dish was held with the left. A fly on the cage 20 cm. away and to the right was caught at but not secured although the movement was accurate. A small piece of apple was wrapped in a piece of paper and placed 75 cm. away, the package was secured immediately and opened in the usual way by teeth and hands. Similarly with other packages.

The animal was killed with chloroform at 11:30 a. m. so that the brain could be examined before there were any marked extensions of the lesions and before any repair could be brought about.

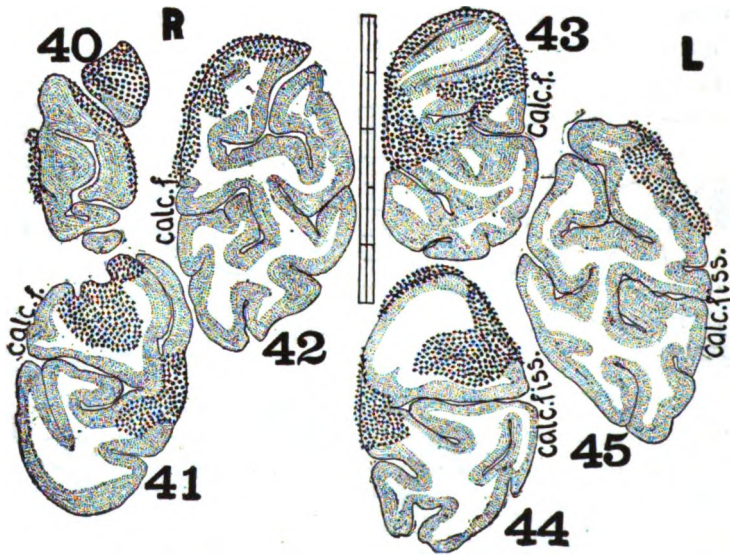


FIGS. 36, 37, 38 and 39. Monkey 3. Lateral and mesial aspects of the cerebral hemispheres, showing the amount of the cortex involved by the operation. Slightly reduced. Traced from photographs.

The autopsy was performed fifteen minutes after death. The wound was healthy in appearance. The brain showed a slight hernia beneath the two skull openings and weighed 102 grams. On account of the brief time between the operation and autopsy,

the brain was examined only for the destruction of cells and the extent of lesion.

Figures 36 to 45 show the amount of the destruction of the cortex. An examination of the microscopical sections (figs. 40 to 45) shows that comparatively large amounts of the fibers were destroyed as well as the cortex, and the cautery undoubtedly penetrated the white substance as well as burning the cortex. It will be seen that the cortex in the neighborhood of the calcarine fissure has entirely escaped, and remained normal, while the greatest amount of destruction took place over the convexity of the hemispheres, being more marked over the superior portion.



FIGS. 40, 41, 42, 43, 44 and 45. Monkey 3. Microscopical findings after the cauterization of the cortex.

The report of Dr. Lafora on this case is as follows: "Monkey 3, Nissl method, frontal sections. On the right side the destruction of the calcarine type of cortex was fairly complete towards the occipital pole except along the calcarine fissure. The calcarine fissure is affected only in its profundity by a reactive process which arose from the injury to the external part of the hemisphere. On the left side the lesions are a trifle more exten-

sive, but the calcarine fissure is affected only in its extreme depth. The external part of the lobe is almost entirely destroyed."

From the accounts of the histological findings and of the clinical appearances of the monkey following the operation, it is plain that the visual defects are not in proportion to the amount of destruction of the so-called visual cortex, and that the lack of blindness can not be ascribed to the failure to destroy the part of the brain which is supposed to have visual functions. This case is to be compared with that of monkey 5, in which latter the calcarine fissure was destroyed more than the remainder of the visual cortex, and the clinical accounts of each of these monkeys differ to an extent to be paralleled by the microscopical findings.

The animal retained its visual function to a very great degree and the disturbance in function can be said to be almost exclusively a dissociation between the sensations from the eyes and the movements of the hand (and other parts). The observations do not indicate a visual defect *per se*.

Monkey 4. This animal had been trained by Dr. Shepherd in his experiments, and about eight months later for a period of about nineteen days it was trained to discriminate blue (sweet) from green (bitter) and yellow (bitter) breads. At this time the habit of taking the blue and leaving the green and yellow was firmly established. No further experiments were done for about seven months at the end of which time the animal appeared to have almost perfect retention of the habit and reacted properly to the conditions as is indicated in the accompanying account.

July 30, 5 tests; in the first test the monkey took the piece of blue bread at first and then the piece of green, but threw the latter away without eating it; the fifth test on this day was made with two pieces each of the three colors arranged in an irregular order and the animal took only the blue breads. July 31, 3 tests; in all tests the animal took only the blue bread; but did not pay any attention to the foods after the third test and the series could not be continued on that day. August 7, 5 tests; in all these tests, after the interval of seven days, the animal took only the blue bread; at first it did not work rapidly but looked around and paid no attention to the breads for several minutes after they

were placed before it; twelve pieces each of blue, yellow and green were arranged on a board in an irregular order and presented to the animal which, after intervals, took ten of the blue pieces and disregarded the yellow and green. August 16, 5 tests; in all the tests the monkey took only the blue bread; in an additional test in which five pieces each of blue, yellow and green were presented at one time the animal took only the blue; it is worthy of note in connection with the discussion of color vision that in the last experiment the yellow bread had been colored by a solution of lead chromate instead of the usual methyl orange, with which it had been previously tested.

Operation, August 17, 2:30 p. m. On the right side an attempt was made to cut away the occipital pole from the rest of the cerebrum by means of the cataract knife; on the left the mesial aspect was cauterized and the cautery was thrust inwards and forwards. The hemorrhage was considerable on the right side, but it was checked by the application of 1:1000 suprarenal solution and by hot sponges. The animal remained lethargic for some time after the operation, but at 4:30 p. m. it was found sitting up in its cage, apparently alert and bright. He appeared to see a hand threatening him no matter from which direction the hand was approached. At 7 p. m. I found the animal had torn the bandage away; after washing the head thoroughly with bichloride, 1:500, I replaced the gauze dressing and bandages and thoroughly sewed them together. An hour later the animal was quiet but appeared to see a small stick thrust at him from right, left, above and below for he moved away from the stimulus.

About twenty hours after the operation, the animal was found quietly sitting in its cage. A piece of bread placed on the floor of the cage was not noticed for about three minutes, but at the end of that time the animal moved toward it, made an accurate movement, picked it up and ate it. A dish of water was placed in the cage, and he immediately moved near the dish, put his head down and drank. Four small pieces of food, one each of bread, cake, boiled potato, and cantaloupe, of about equal sizes were placed on the floor of the cage; the animal selected the cantaloupe first, then took the cake and the potato and left the bread. In four similar tests, the same result was obtained, but it was noticed

that at times the monkey took the bread but threw it away without attempting to eat it. The movements of grasping were usually made with an error of about 3 cm. In these experiments the animal did not always take the food in its hand from the normal sitting posture, but usually lowered its head toward the food before reaching for it. It was concluded that this indicated an impairment of vision, and the inaccuracies in hand adjustments for the grasping of food were taken as confirmations of this conclusion; the impairment seemed to be not a blindness, but only a dimness of vision.

Twenty-six hours after the operation. Two sticks were simultaneously introduced into the cage, one to the right and the other to the left of the animal; that on the right attracted the attention of the animal and was seized, that on the left was not seized even when it was advanced to within a few inches of the head of the animal. One of the sticks was held near the floor, the other above the level of the eyes; the animal reached for the upper stick and disregarded the lower one until it had been moved so as to touch the body. The movements of grasping the stick on the right were as accurate as before the operation, those of grasping the stick above the level of the eyes and those near the floor were very inaccurate. The colored breads, when presented to him, were disregarded and the test of this visual function was unsuccessful. The animal was able to see food and other objects on the floor of the cage but it was not determined in what manner this was accomplished. From the peculiar movements of the head, the lowering and the turning, it appeared to be necessary for the animal to get the visual image on the lower retinal segment, for, although an object lying on the floor was approached, it was not taken until the head had been lowered and the eyes turned downwards. The direction of the errors (right and left) in taking the food were not recorded.

Two days after the operation, the animal went into the small observation cage as soon as the door was opened. At that time the animal was more lively than it had been and its actions were apparently normal. It picked up food lying upon the floor of the cage. When the hand was held above the cage the animal showed signs of fright, shrinking into a corner, etc. A piece of

pear held above the level of the eyes was seized quite accurately. Two pieces of pear were held simultaneously on a level with the eyes, one to the right, the other to the left, each about 10 cm. in front of and to the side of the median line; the animal always took the piece on the left first, sometimes with the right hand, sometimes with the left. A rod and a piece of pear were held simultaneously in the upper part of the field; when the rod was on the left it was seized and the pear was disregarded; when the pear was on the left it was seized and the rod disregarded. A rod held about 15 cm. below the level of the eyes was disregarded until the head was moved or until the rod touched the body or the hair. The animal peered through the opening at the back of the cage with all the appearance of the investigation of a normal animal.

On the same day, four hours later, the animal was tested and examined by Dr. Lafora, who reported as follows: "Pupils react well to light. Held yellow and blue breads before the animal twice, and each time the animal took the yellow bread first and the blue afterwards. A piece of melon and blue bread, the latter nearer the animal were presented simultaneously, and the monkey reached beyond the blue bread and took the melon. Two pieces of melon (one held in the visual axis and the other to the right, left, superior or inferior) were presented simultaneously, and each time the animal took both pieces, apparently seeing with all parts of the visual field. For testing the ability to see in the lower part of the visual field a board was placed near the bottom of the cage so that food in the hand could be kept out of sight until it was time to show it; in the right hand a piece of food was held in front of the animal in order to attract the attention, but just beyond the reach so that the animal would need to exert himself in his efforts to obtain the food, and a second piece was held in the other hand behind the board; the attention of the monkey was gained by the exhibition of the piece of food in the right hand, but when the second piece was exposed, the animal reached for the latter. Two tests were made with pieces of green and white breads after having performed many tests with melon and white bread; in the later tests the animal always reached for the green bread; in tests with green and blue breads in which the green

bread was held nearer the animal than the blue, the monkey always reached beyond the green bread and selected the blue bread. When, however, the blue bread was held in the inferior and the green in the superior part of the field the green was always taken. When a piece of melon and a piece of green bread were shown simultaneously the animal always selected the green melon. Under the following conditions the animal always selected the vegetable or fruit instead of the bread; blue bread and tomato; blue bread and melon; green bread and tomato; green bread and melon; even when the bread was nearer or farther from the animal, to the right or to the left, and when one or more pieces were presented simultaneously. Repeated tests to determine whether or not both right and left visual fields were normal showed that the animal made grasping movements for food regardless of the part of the field in which the food was presented, but that the movements for food on the right were not as quick as those on the left. When pieces of melon or tomato were held in the hand close to the cage the animal always took them, but did not take the green, red or yellow breads."

Four days after the operation. When a plum was thrown into the cage the animal moved toward it and tried to pick it up but failed several times to locate the fruit. At first it grasped another piece of food which it then smelled and rejected, and continued to seek the plum until it was found.

Five days after the operation. A paralysis of both hands was found, the fingers were shut into a fist and there was a wrist drop. The movements of the arm appeared to be normal, and it was found that when the arms were extended the fingers were voluntarily moveable to a slight extent. When a pear was rolled on the floor of the cage the animal moved toward it and attempted to get it in his hands but on account of the flexion of the fingers could not do this; the pear was then eaten by the animal moving its head close to the pear. The pear and other pieces of food were held to a slight extent by the thumbs of both hands, which appeared to be more moveable than the fingers.

On the following day (six days after the operation). the paralysis of the hands disappeared; the movements of grasping and of holding were normal in character, and, judging from the force

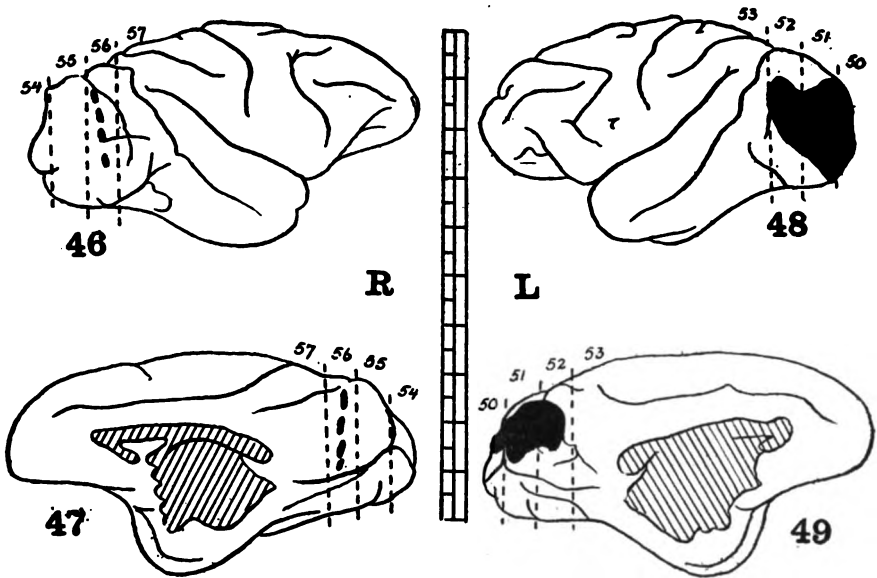
with which it held a rod inserted into a cage, of good force. The monkey did not eat any bread presented to it, and the colored breads were disregarded; on this account a test of the color discrimination could not be made. All fruits and vegetables were rapidly and unerringly selected from the food presented to him so that it was evident the animal had considerable discrimination ability. The animal was not fed at this time and in the afternoon tests with the colored breads were made. In two tests the monkey selected the blue bread and disregarded the yellow and green, but on the third test the animal picked up at one time both the blue and the green, disregarding the yellow.

Two days later (eight days after operation). Tests with the colored breads; in three tests took blue bread and in the third took yellow after having eaten the blue, but did not eat the yellow; in a fourth test in which two pieces each of yellow, red and blue breads were presented simultaneously in irregular order, the monkey took the blue pieces, brushed the red pieces off the plate and disregarded the yellow bread. Tests with small pieces of bread, cooked potato, raw apple, cooked green beans, and raw tomato resulted in the selection of the vegetables and the disregarding of the bread.

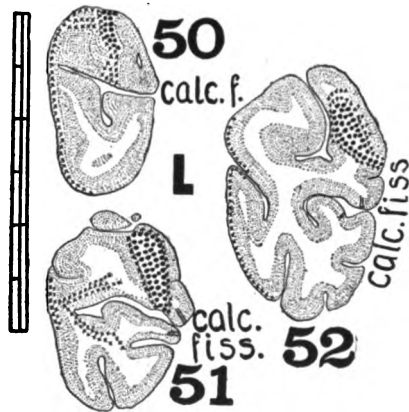
Ten days after operation. The animal was found in a convulsion with twitchings of arms, head, eyelids, etc. The fingers were flexed as they had been five days previously, and there was inability to grasp food; the condition passed away by the following day, and the animal caught with accuracy grapes rolled toward him on either side and placed them in his mouth. Other foods were selected from bread, their positions were accurately judged (as evidenced in the accuracy of the movements) and they were eaten.

Five days later (fifteen days after the operation). Color tests; took only blue bread. The animal was killed on this day in order that examinations of the brain might be made by the methods of Marchi.

Figures 46 to 57 show the results of the post mortem examinations. In figs. 46 to 49 are shown the results of the lesions. The extent of the cortex destroyed by the cautery was accurately determinable but on account of the character of the operation on

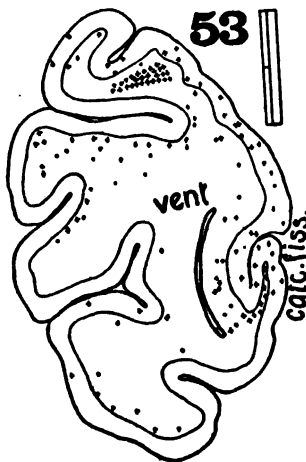


FIGS. 46, 47, 48 and 49. Monkey 4. Lateral and mesial aspects of the cerebral hemispheres, showing the extents of the lesion of the cortex on the left side, and the approximate course of the knife cut on the right. Slightly reduced. Traced from photographs.



FIGS. 50, 51 and 52. Monkey 4. Frontal sections of the left hemisphere in the neighborhood of the cauterization, showing the amount of destruction. Slightly enlarged. Drawn with Edinger projection apparatus.

the right side it was not possible to determine the amount of the visual area involved by the operation. The approximate location of the knife cut on the right side is shown in the figure. The results shown in figs. 50, 51 and 52 are due to the cauterization of the cortex, and have been investigated by the Nissl method. In parts it will be noted that the depth of the cortical destruction

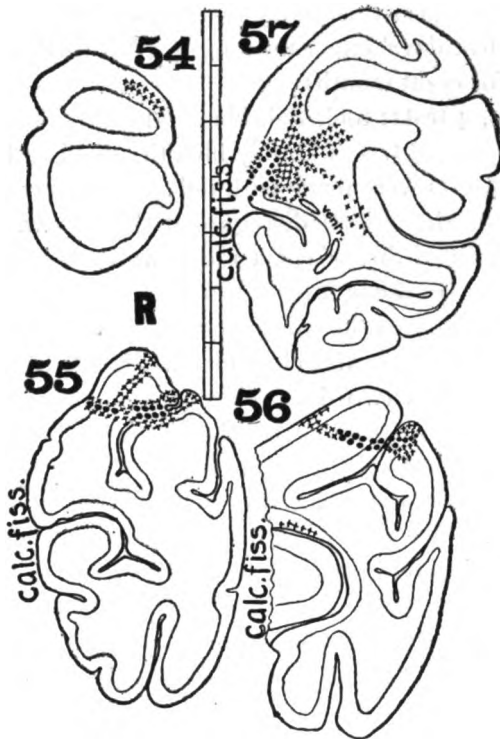


FIGS. 53. Monkey 4. Vertical section through the left hemisphere, anterior to the cauterized area. Few fat drops are indicated in the diagram by crosses. Slightly enlarged. Edinger drawing apparatus.

has not been great, a matter which has been considered in an earlier portion of this article. The section of the left hemisphere anterior to the parts examined by the Nissl method was examined for fiber disintegration, or degeneration, and this showed, figure 53, a rather diffuse process, but with no marked evidences of degeneration. The examination of the right hemisphere by the Marchi method was unsatisfactory, for the only effects of the lesion were those in the immediate neighborhood of the cut.

Regarding these preparations Dr. Lafora has written: "Monkey 4. Cauterization of the left hemisphere, cutting of the white matter of the right hemisphere. Frontal sections. Nissl method on the left, and Marchi method on the right. On the left the calcarine fissure has almost entirely escaped destruction. The outside of the brain (the calcarine type of cortex) is affected in the external layers up to the granularis interna. (On account of

the time of death so soon after the operation the cortex showed only the primary effects of the operation and not the secondary ones.) A large hemorrhage extends over the calcarine type of cortex on the lateral aspect of the hemispheres and extends into the white substance. Sections of the brain anterior to the cauterized area were examined by the Marchi method, and these showed some fat drops as are illustrated in the figures. The right side was placed in toto by mistake into the Marchi fluid which pro-



FIGS. 54, 55, 56 and 57. Monkey 4. Frontal sections of right hemisphere, showing destruction in the neighborhood of the knife cuts. For fuller description, see text. Slightly enlarged. Edinger drawing apparatus.

cedure did not permit a proper impregnation, consequently the appearances found in the sections are only those near the surface. Here are found many fat drops around the hemorrhagic areas, which were produced by the cutting."

Clinically, it appeared at first as if this animal had a visual defect but later observation failed to indicate this, and the conclusion is forced upon us that if there was a defect during the first few days it was minor in character, and not of sufficient amount to be called amblyopia, hemianopsia, or anything more than what Loeb has called a "reduction in irritability."

Monkey 5. In the preliminary experiments this animal had been trained to take red and blue breads and to disregard the yellow and green. These early experiments were made on eighteen days over a period of sixty-one days, the habit being established after five days' experimentation. No further experiments were made for eight months.

August 19, 4 tests; took red, blue and green the first test; red and blue the second test, and only blue the third and fourth tests. August 20, 5 tests were made with only red, yellow and blue, and in all the tests the animal disregarded the yellow. August 23, after an interval of three days, 10 tests were made with blue, yellow and green. In the first test the animal took the yellow as well as the blue, but in the other nine tests took only the blue. August 25, 10 tests; took only the blue and disregarded the yellow and green. On the last two days one test each was made in which two or more pieces of each of the three kinds of bread were presented simultaneously, and in these tests the animal selected the blues from the other colored breads.

August 26. Operation. Both occipitals were cauterized, the cautery being inserted into the brain substance as well as being passed over the cortex.

Three hours after the operation the animal appeared to be fairly normal. He picked up pieces of apple placed on the floor of the cage, the color of which was quite similar to that of the food. The movements were accurate when the food was placed within 30 cm. of the animal, and in addition the animal made accurate movements to secure food held at the top of the cage.

Four and a half hours after the operation, the animal was observed by me in conjunction with Dr. I. W. Blackburn. At this time the following notes were made: The pupils reacted to light in a normal manner, and the general appearance of the animal was normal. He peered around the cage and through the opening in

the back, and examined all things placed in his way. In his usual manner to strangers, he threatened Dr. Blackburn, but afterwards remained huddled in a corner, but moved whenever the hand was inserted into the cage or when food was presented. Grapes rolled toward the animal were immediately and accurately seized; grapes held in the hand 30 cm. distant were reached for but not accurately grasped, for at times he took hold of the experimenter's finger. A grape rolled to the other end of the cage (75 cm. distance from the animal) was disregarded for ten or fifteen seconds, although the animal was hungry and ate anything placed within its reach. Grapes and pieces of apple placed near enough to be reached were easily grasped, but at times with slight incoördination amounting to about 4 cm. The grasping movements were sometimes to one side, at other times to the other, and often nearer the animal than they should have been. A mirror placed in the cage attracted the attention, the animal moved toward it, peered into it and examined the front and looked behind it repeatedly. He took a piece of red colored blotting paper and inserted it into the mouth, but then rejected it. A small piece of bread about 3 mm. in diameter was picked up from the floor, but as he continued to fumble for food and at times picked up grape skins which he had previously rejected it was impossible to decide whether the small piece of bread had been seen or had been hit by the hand as the animal moved the hand over the floor. The monkey repeatedly took the grape skins which it had previously rejected, placed them in the mouth and immediately spat them out again. A pair of eyeglasses were held so that four images of the electric lights were thrown on the floor of the cage; these attracted the attention of the animal and he moved his head and eyes in accordance with the movements of the images.

Four days after the operation. It was noted that when food held on wires was presented to the animal, the food held in the upper left visual field was always seized immediately, but that the food held in other parts of the field was sometimes seized and sometimes not seized. On this day an attempt was made to test the memory for colors but the animal would not touch the breads and the test could not be made.

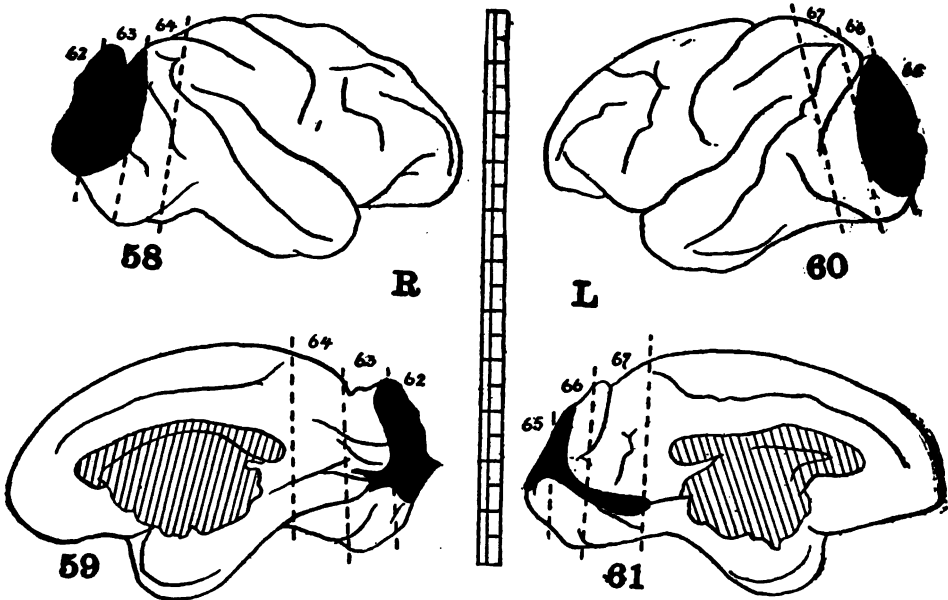
On the following day, five days after the operation, the tests

with colored breads on the glass plate were also unsuccessful, and the breads were therefore presented in pairs on wires, viz., blue and yellow, and blue and green. In ten tests with the yellow-blue pair the monkey took the yellow once, the blue four times, and both pieces five times. The latter result was obtained after the other five. With the green-blue test, the monkey took the blue five times, and did not take the green. On this day a lump of sugar was placed at the bottom of the cage but it remained unnoticed although before the operation the animal was very fond of sugar. Other pieces of food on the floor of the cage were also disregarded, although when food was placed close to the nose it was seized and eaten with eagerness. A broom and a stick, of which the animal was normally afraid, were introduced into the cage without producing any reaction on the part of the animal.

Six days after the operation the tests with colored breads were more successful. Five tests were made with the following results: In the first test the animal reached in the direction of the blue, but slowly drew its hand backwards and then put its head down to the food plate and took the blue in the mouth. In the second test, the animal fumbled about the plate, finally hit the yellow bread, which it placed in its mouth but immediately spat out. In the third test he grabbed for the bread, caught the green piece, which was tasted and thrown away, and then he took the blue which he ate. In the fourth test, he took the blue bread immediately, ate it and disregarded the other two pieces. In the fifth test he took the blue, but only tasted it; the other pieces were not touched. Following these experiments he was placed in the cage with another animal. As soon as he got into the other cage he walked half way across the cage and picked up a piece of food lying on the floor, and then stood up and reached for a piece of apple on the wire netting at the top. Small pieces of apple were then placed at the top of the cage to the right or left of the animal, and in every case he accurately reached for the food. In one test in which two pieces were placed about 10 cm. apart, he reached for one piece, but in taking hold of the netting the apple dropped to the floor of the cage whereupon he immediately changed the direction of the movement and secured the sec-

ond piece. After having secured the second piece he accurately reached for and secured the first piece which had fallen to the floor.

During the next three days the inaccuracies in reaching for food increased, until the animal was able to get food only by fumbling around on the bottom of the cage. Eventually his movements became like those of a blind person. Ten days after the operation it was noted that the animal had all the appearance of being blind. He paid no attention to the movements of the experimenter's hand and arm or to a small flag. Food held at a distance of 30 cm. was unattended to except when held in the upper left part of the visual field. He reached for food when it was placed on the floor of the cage but the movements were inaccurate and it appeared that the animal did not appreciate

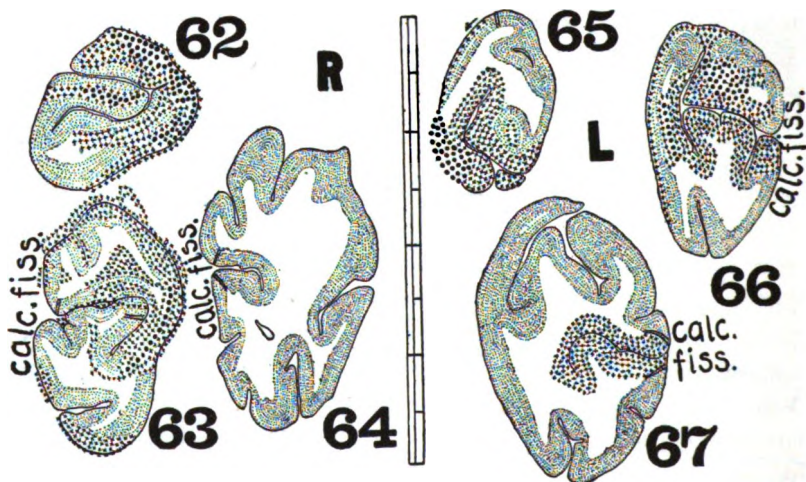


FIGS. 58, 59, 60 and 61. Monkey 5. Lateral and mesial aspects of the cerebral hemispheres, showing the extent of the lesions produced at the operation. Slightly reduced. Traced from photographs.

what he was reaching for. Once a small bright red tomato about 2 cm. in diameter was exhibited in the upper left part of the field;

the monkey reached for it in a fumbling manner and knocked it down so that it rolled to the left and behind him. He sought for the food through the bars of the cage, although none was visible, and groped hither and thither. When the tomato was shown a second time in the upper left field, he reached for it, making an initial error of about 10 cm., but finally managed to secure it. Black and white grapes rolled on the floor of the cage were not taken although the animal seemed eager for food. Grapes held near his nose were taken hold of accurately. In the tests made on this day it should be mentioned that there was an intention tremor and part of, if not all, the inaccuracy of movement, may have been due to this motor derangement.

Eleven days after the operation the animal was tested, and found to be in almost the same condition as that noted in the preceding paragraph, except that there was less ability to grasp things presented to him. He was killed on this day. At the



FIGS. 62, 63, 64, 65, 66 and 67. Monkey 5. Frontal sections of the cerebral hemispheres, showing the parts affected by the operation. In 64 no lesion was found. Edinger drawing apparatus. Slightly enlarged.

autopsy the brain was found bulging above the level of the bone, the hernia on the right side being more marked than that on the left. There were adhesions between the brain and dura and scalp. The brain weighed 92 grams.

The results of the post mortem examinations in this animal showed much more extensive lesions than in any other animal upon which I operated. The visual defect was noticeable only after four or five days (although there was some incoördination from the first) and in this case it is apparent that the end result, the apparent blindness, must have been due to the extension of the effects of the primary lesion. Only the anterior part of the calcarine fissure on the right side was preserved (see figs. 59 and 64) and the preservation of this, with the apparent lack of foveal vision is against the hypothesis of Henschen.

The results of the histological examination by Dr. Lafora are as follows: "Monkey 5. Cauterization of the cortex. Frontal sections, Nissl method. On the right side, with the exception of the most anterior portion of the calcarine fissure, which is a very small area, all of the calcarine type of cortex has been destroyed, even the external part of the occipital pole, and large parts of the white substance in the neighborhood of the calcarine fissure. On the left side the lesion was apparently selective, for the effect is localized along the calcarine fissure throughout its entire extent, and into its depth. The occipital pole has only the outside part of the cortex destroyed."

Monkey 6. This animal was trained to discriminate red and green breads. The training results were as follows:

August 22, 5 tests; in all tests the animal took both kinds of bread. August 24, 10 tests; after the first four tests the animal did not eat the green, although on the sixth test she picked it up but discarded it without smelling or tasting. August 26, 5 tests; in all of which the red was taken and the green was not taken. August 27 and 28, 5 tests each day; in all the green was disregarded and the red taken. August 31, 5 tests; took only red although in the last test on this day four pieces of red and six of green were presented simultaneously. September 1, 5 tests; in all of which the red was taken and no green. On this day the operation was performed.

Operation, September 1. After the trephine buttons were removed the openings of the skull were enlarged by means of bone forceps so that all of the lateral parts of the occipital lobes were exposed. At the time of the operation the hemispheres were

separated by a glass spatula and the internal aspects of both hemispheres were cauterized as well as possible. The injury in this animal was apparently more extensive than in animals 3, 4, and 5. A hemorrhage from one of the large occipital arteries was checked by an application of suprarenal, 1:1000.

A half hour after the operation the animal was found sitting in its cage, apparently looking at everything passing, for she followed me with her head and eyes when moving around the room.

Two hours after the operation, she was found peering through the opening at the back of the cage, and when I quietly moved toward this part she retreated to the other side of the cage. When I was seated in front of the cage and made movements with my hands and arms, she followed with her eyes all the movements in a perfectly normal manner. The reflexes of the pupils were normal. Four pieces of white bread were put into the cage, the animal appeared to look at these, and she touched some and carried one to her mouth, but did not eat it. A grape rolled into the cage among the pieces of bread was instantly seized and placed in the mouth. Three grapes were placed among the bread and all were selected and the bread disregarded; in these tests it was noted that the error of movement was from one to three cm. Pieces of muskmelon were selected unerringly from the bread of the same sizes. Only two pieces of pear were taken although six were placed in the cage. When pieces of melon, bread and pear were presented the monkey took the pieces of melon and left the bread and pear, and when grapes, melon, pear and bread were presented she took only the grapes.

Five hours after the operation she was observed by Dr. Lafora, who made the following observations: "Color vision was tested by the ability of the animal to discriminate foods of different colors; red bread from a red grape, melon from green bread, and combinations of different kinds of food. In these experiments the monkey took the fruit in preference to the bread, but she did not appear to be hungry and after about ten minutes would not attempt to secure food. Tests of peripheral vision by means of food placed at the ends of wires showed that when the food was in any part of the visual field it attracted the attention of the animal and there were no results that could be taken to indicate a

contraction of the visual field, nor any difference in sensation ability."

One day after the operation, when sitting quietly in front of the cage observing the actions of the animal, there being no food in the cage, I saw her make a quick movement in the air thereby catching a fly on the wing. At this time, the monkey did not work with colored breads, entirely disregarding them as they were presented on the glass plate. Throughout the period of observation on this day, the animal kept picking at the edges of the bandage and when she had managed to get a small piece of cotton she would hold it about 10 cm. in front of her eyes, as if carefully examining it; at times she placed the cotton in the mouth and eventually threw it away.

During the next five days there were no new observations worthy of note, the animal appearing to be in much the same condition as was noted the day following the operation. On the sixth day after the operation it was first noted that the animal did not appear to see objects placed on the floor of the cage for she left fruit and other kinds of food although she appeared to be hungry and took all things presented to her in the hand or on wires. On this day the visual defect was like that of many of the animals, indicated by an inaccuracy in movements in grasping the particles of food, for the errors in movement amounted to from one to four cm. On this day, however, four tests with colored breads were made, and in these tests she picked up the red bread and disregarded the green three times; and in the fourth trial she moved in the direction of the red but managed to pick up the green which was only two cm. away from the red.

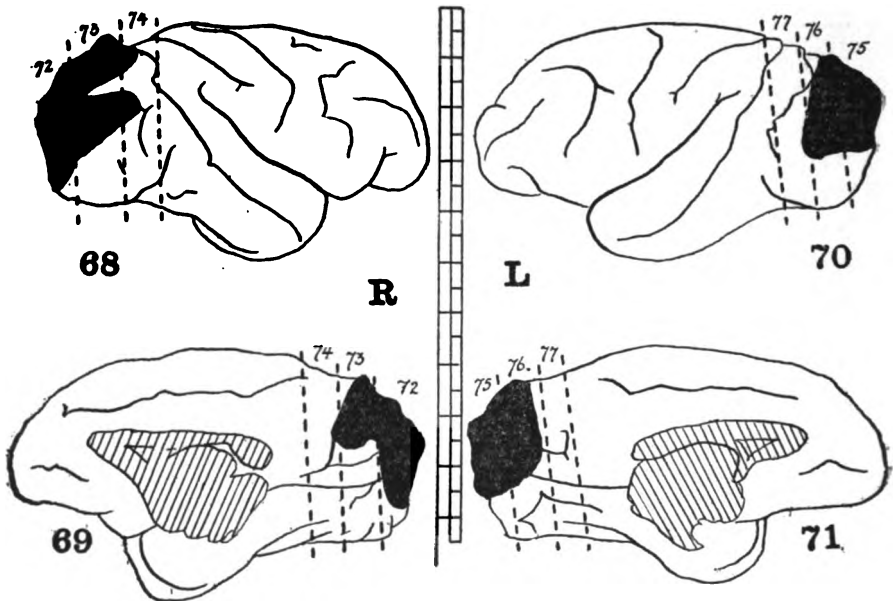
Two days later, eight days after the operation, three grapes were dropped into the cage; they rolled into positions forming an irregular triangle with the apex about 50 cm. from the eyes of the animal. The monkey made repeated efforts to obtain the grapes but at first got its hand 15 cm. too close to its body and then fumbled around until she by chance hit one of the grapes; this grape rolled closer to the animal but the monkey continued to feel around in the spot at which the grape had been felt and at this time she did not manage to get this grape or the other two which were presented. Afterwards with considerable fum-

bling she secured the two grapes the positions of which had not been disturbed and finally the one which had been moved by her fumbling, and which, as has been said, rolled closer to her body. A second set of observations were made on this day with somewhat similar results. The defect of vision appeared to me to be general and not of any particular retinal segment. When two pieces of food were held on wires one to the right and the other to the left, or one above and the other below the line of regard both pieces were secured, but only after considerable fumbling and after making numerous futile attempts to reach the food. Grapes rolled into the cage were searched for and secured only after they had been hit by one of the fumbling hands in their apparently rambling movements. Seven lima beans were placed on the glass plate in the form of a hexagon, and only after much fumbling were these obtained. A small very bright red tomato, placed 60 cm. from the animal, was not noticed for two or three minutes, but after that time it appeared to me that the animal had sensed its presence, whereupon she began a search for the tomato finally securing it after about 40 seconds. Grapes and beans lying outside the cage were secured with difficulty; usually the animal made a movement in the general direction of the food and then, not finding it, swung the arm and hand until something was touched. In taking food presented at the ends of wires and from the floor it was noted that the first movement was in the general direction of the food, and when the food was not secured by this first movement (as was usually the case) the monkey drew the hand backwards, peered at the food for a second or so and tried again. The errors of movement (i. e., the distance between the food and the tips of the searching fingers) averaged about 2 cm., and the record of nine different attempts show that these errors were in all possible directions, too far from, too near, to the right of and to the left of the food.

On the succeeding days the inaccuracies in adjustment became less noticeable, and thirteen days after the operation they were to be discovered only by careful observation. On this day the animal was examined by Dr. F. M. Barnes, Jr., who wrote the following notes; "The animal is quite active, moving about the cage and taking notice of all about it. She does not seem especial-

ly frightened but moves quickly at very slight noises. It seems impossible to make the slightest movement in front of the cage without attracting the animal's attention. When a pencil is pointed at her through the wires of the cage she strikes at it with her right hand; the aim is true and sometimes she grasps the pencil and pulls it. Vision for objects (wires and rods) on both sides and from above and below is acute. A piece of potato was placed under a watch glass and introduced into the cage. The animal grabbed for it several successive times, then with both hands found the edges of the watch glass and made an attempt to turn it over. A piece of potato was placed in a small glass crystallizing dish, which the animal seized, turned over quickly but did not use the hand to secure the food but inserted the mouth. Another trial under similar conditions, but the potato dropped to the floor when the glass was introduced: the monkey picked up the dish, held it to the mouth and tried to get food from it leaving the food on the floor unnoticed. When the dish and the watch glass were placed in the cage she took up both even though no food was in them. Blocks of apple and carrot thrown into the cage are generally picked up quickly and accurately. Often she seems to look directly at a piece of food for some time before taking it up. She frequently misses a piece of food by 5 to 8 cm., and on groping gets farther off. With several different kinds of food thrown into the cage she does not appear to discriminate but picks up ginger snap along with the apple and only after having conveyed the ginger snap to the mouth does she reject it. She often picked at a knot hole in the floor of the cage and appeared to attempt to pick it up. Vision does not seem disturbed when tested by rods approached from the four retinal quadrants: the upper segment may have some dimness, for there is some inability to discriminate and to pick up food from the bottom of the cage. There is some incoördination of movement in prehension. It appears as if the animal does not discriminate between the colors, white, red, and brown." The last conclusion does not appear to me to follow from the results of the experiments, for although the animal in its normal condition took fruit in preference to bread and ginger snap both the latter kinds of food were eaten with apparent enjoyment. It also appears that

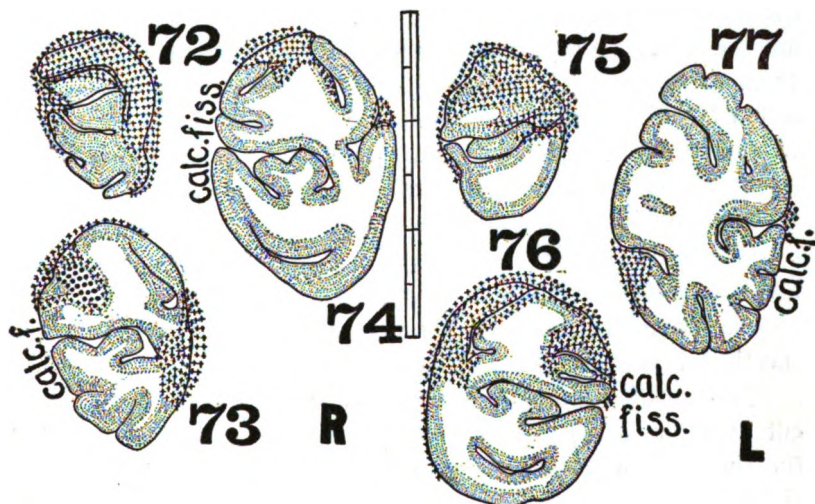
the incoördination of movement in grasping the particles of food may have given the impression that there is no ability to discriminate, for in tests with other animals it was found that when pieces of food were widely separated the movement was always made in the proper direction, although the food was not immediately secured, but that when the food pieces were close together the animal at times managed to get the wrong piece and often to take two pieces at once. It would perhaps be better to make a statement of the action rather than to draw the conclusion: the animal picked up indiscriminately pieces of white, brown and red food, although when these pieces were conveyed to the mouth only the red were usually eaten.



FIGS. 68, 69, 70 and 71. Monkey 6. Lateral and mesial aspects of the hemispheres, showing the extent of the lesion produced by cauterization of the pia and cortex. Slightly reduced. Traced from photographs.

The animal was killed two days later, without having shown any further variations in vision or other sensory functions. At the autopsy, the brain was found adherent to the dura and to the scalp over the site of the trephine buttons, and it was with diffi-

culty separated from the scalp. The brain weighed 94 grams. The occipital lobes were placed in alcohol for examination by the Nissl method and the remainder of the brain was hardened in bichromate for examination by the method of Donnagio.



FIGS. 72, 73, 74, 75, 76 and 77. Monkey 6. Frontal sections of cerebral hemispheres, with drawings of the histological examination. Slightly enlarged. Edinger drawing apparatus.

The results of the operation are to be found represented in figures 68 to 77. It will be seen that in this animal the cortex surrounding the calcarine fissure has not been disturbed to any extent and that the destruction of the visual cortex beyond the calcarine region has been very extensive. Such a lesion should have produced a very decided visual defect if this area were concerned with perceptions or sensations of a visual character, for with the exception of the cortex along the calcarine fissure practically all the so-called visual cortex has been affected.

The report of Dr. Lafora on the examination of the microscopical sections is as follows: "Monkey 6. Cauterization of the brain, frontal sections, Nissl method. On the left side the cortex along the calcarine fissure is not affected excepting the oral side of the superior ramification, at the extreme end of the occipital lobe. The lesion on the right closely corresponds with

that on the left, but there is a slight hemorrhage near the calcarine fissure." The examination by the method of Donnagio has not yet been completed.

Monkey 7. This animal was trained to discriminate the four colors. At first the animal did not seem to discriminate between the bitter breads, and for the first eight days of the experiments ate the bitter (green and yellow) breads, as well as the sweet (red and blue), even though at times the green and yellow were rolled in quinine and the animal had previously been thoroughly fed and the cheek pouches were filled with food. Oct. 27, the eighth day of the experiments, she refused the green and yellow breads most of the time, and on the following three days did not take the bitter breads once. On Nov. 1 and 3, she took the green bread once each day, but in later tests (November 5, 20, 25, and 28), after intervals of one, fourteen, four and two days, respectively, she made no mistakes.

The operation was performed Nov. 28. Both occipitals were cut away by a frontal incision. On cutting the left occipital, the right eye was observed to rotate slowly upwards and to the right, and this movement appeared to be concomitant with the slow drawing movement of the knife. The monkey reacted well after the operation, and two hours after having been placed in her cage was noticed to be busily engaged examining all parts of it. Her movements were rather slow, but they seemed to be executed well and precisely. No reaction to noises, loud or soft, e. g., whistling, clapping hands, etc., was observed, but anything moving about the cage attracted her attention. The observation cage had been placed in a room to which the animal was not accustomed, and she repeatedly went to the 5 cm. opening at the back and peered through it. Eight hours later the animal was found asleep, but when awakened seemed to be in the same state as that just described, curiously examining all things, following with the head and eyes moving objects, etc.

One day after the operation, a few tests with colored breads were made. In these the animal selected the red and blue and disregarded the green and yellow, but in each test did not take both sweet pieces. The movements were quick and accurate. She picked up food from the floor of the cage, accurately reached

up to the top of the cage and secured small pieces of sugar and other food which were held there. She was tested with the colored breads with the same result noted above. She was then placed in the cage with the male Monkey 8, whom she picked over for fleas, dandruff, etc., in a perfectly normal manner. During this procedure she was seen to stop, and make a quick movement, thereby catching a fly.

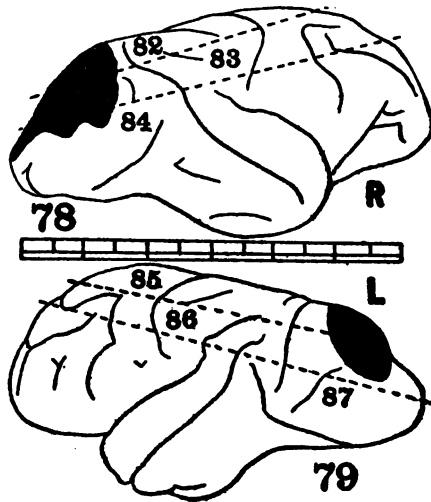
Two days after the operation (Nov. 30), ten tests with the colored breads showed perfect retention of the ability to discriminate, and on the following day (Dec. 1) the following results were obtained: Tests 1-6, ate all four pieces; 7, ate the red, blue and yellow, and disregarded the green; 8, ate red, blue and yellow, but did not pick up the green; 9, and 10, ate the red and blue, but did not take the yellow and green. Two days later she took red and blue in the first trial but refused to take more, and on the following day (Dec. 4) refused to work. Two days later in ten trials she ate all four pieces presented to her.

During the succeeding days no abnormality of vision was detected; the movements of grasping objects presented in any part of the visual field were accurate and quick, and there were no observable errors in discrimination. A second operation was determined upon but deferred for about four months and a half (April 16). During the intervening period the monkey was returned to the large cage with a normal monkey, and so far as it was observed acted in a perfectly normal manner. At the end of about four months the memory for the colored breads was tested and found to be defective. On the first day after this second series (March 29), she took all four pieces in the five tests, but two days later made only two mistakes in ten trials. During the next two weeks the monkey was given tests on three days, with intervals of 5 and 7 days respectively, in all of which the green and yellow were disregarded and the red and blue were taken. On the day of the second operation (April 16) the monkey took the yellow bread several times and the green once in ten trials.

Second operation, April 16. At the operation the posterior portions of the occipital cortex were cauterized. The site of the trephine openings made at the time of the first operation

were distinctly visible, the button on the right side was soft, that on the left was hard. New openings were made slightly posterior to these, and the old buttons were removed. It was found that the bridge of bone between the two original trephine openings had disappeared, so that when the new trephine holes were made, both occipitals were exposed and the bone opening extended from one side to the other. The loss of blood at the operation was slight and the animal recovered from the anesthetic soon after being taken from the operating table.

Fifteen minutes after the operation the monkey sat up in the

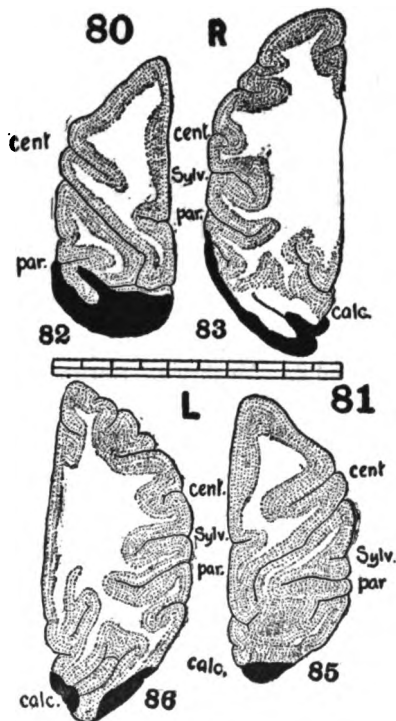


FIGS. 78 and 79. Monkey 7. Lateral aspects of cerebral hemispheres, showing the extent of lesions produced at the second operation, and the location of parts examined by histological methods. Slightly reduced. Traced from photographs.

cage, looked about in a dazed manner and moved about slowly. An hour after the operation she was more nearly normal. She took a piece of bread presented to her through the wire netting, moving forward about 25 cm. and reached for the food with an error of only about 3 cm. One hour later she was given a peanut, which she reached for accurately. She conveyed the peanut to her mouth, took the nut out of the shell and began to strip the red skin from the nut in a normal manner. When the shell

had been thrown away, the monkey took the nut from her mouth from time to time when stripping the skin from it and examined it until the skin had been removed. She was given water in a saucer, but did not drink it for an hour.

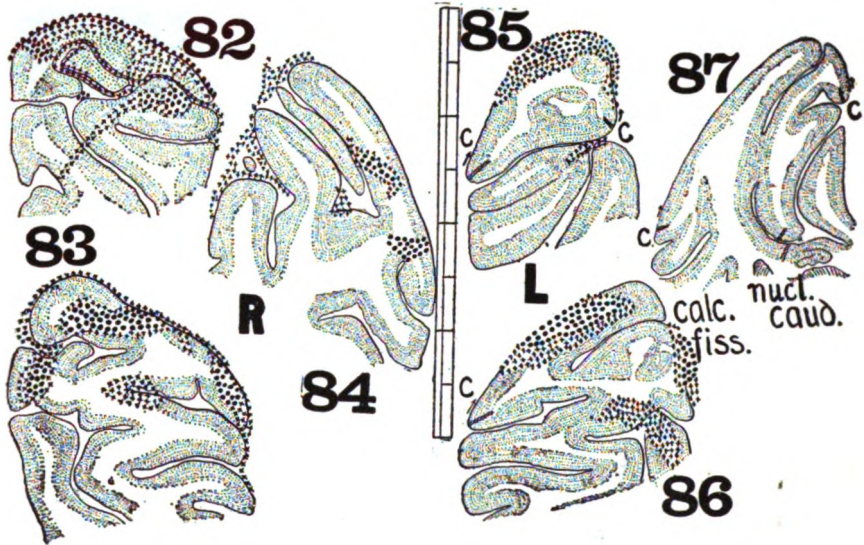
The day following the operation, the animal appeared to be normal, and two days after the operation in five tests with colored breads she showed perfect discrimination. At no time was there evidence of a visual disturbance as the result of this operation. The monkey was taken to Baltimore, April 20, and exhibited at the meeting of experimental psychologists, after which it was killed.



FIGS. 80 and 81. Monkey 7. Tracings from photographs of lower parts of sections 82, 83, 85, and 86, of figs. 78 and 79, showing extent of lesions. Slightly reduced.

At the autopsy the areas of softening of the cortex were distinctly noticeable apparently extending over the convexity of the

occipital lobes almost to the parieto-occipital fissure. The results of the autopsy and histological examinations are shown in the accompanying figures (78 to 87). In these it will be seen that the occipital poles have been almost entirely destroyed, and that the destruction of the so-called visuo-sensory cortex is extreme,



FIGS. 82, 83, 84, 85, 86 and 87. Monkey 7. Horizontal sections of cerebral hemispheres, with results of histological examinations, showing the amount of destruction by cauterization. Slightly enlarged. Edinger drawing apparatus.

with the exception of that surrounding the calcarine fissure.

The report of Dr. Lafora on the brain is as follows:

"Monkey 7. Cauterization of the brain: Nissl method; horizontal sections. On the right side the calcarine area is affected only at the superior branch of the fissure, but the cortex at the occipital pole is almost entirely destroyed. On the left side there is a marked destruction of the most posterior part of the occipital pole with an irritation lesion of the pia around this lesion. In the superior branch of the calcarine fissure there are some lesions." The effects of the first operation (section) were not determined by the histological method employed, and at the time of writing no examination of fibres had been made.

Monkey 8. This animal was trained to discriminate red and yellow breads. The training began Oct. 20 and continued until Nov. 26, with intervals of 2, 6, and 11 days—after the habit had been formed. At the latter date, the animal had indicated the ability to discriminate the red from the yellow, and to retain this over a period of eleven days.

The operation of cutting both occipitals was performed Nov. 26. There was considerable hemorrhage which was controlled by hot compresses. There was also considerable shock from the operation, and the monkey did not recover from the general defects for several hours. Immediately after the operation it was noted that the pupils were slightly dilated, but were equal. A slight nystagmus was suspected but at times this did not appear to be present, and it was impossible to decide whether or not the movement was partly voluntary. The eyes appeared to be rotated toward the right. When placed in the observation cage the animal was laid on the left side but it kept its head raised from the floor, and there was a slight tremor of the head. Three hours after the operation the animal appeared lethargic, its general movements were slow and apparently performed only with great effort and without much force. When a stick was thrust through the wire netting the monkey reached for it slowly but accurately. It grasped the stick, carried it toward its mouth and bit it, making these movements slowly and pulling and biting the stick without any display of energy. The wound or the bandage was apparently irritating, for the monkey continually scratched at the bandage when its attention was not attracted by and directed to objects held by the experimenter. The animal grasped food accurately but slowly, and picked up food which was placed on the floor of the cage, even when at the farther end (about 75 cm. distance). When a prune was held 10 cm. in front of the cage, the monkey put its hand through the mesh of the wire netting slowly but accurately, grasped the prune, drew it in, and immediately ate it.

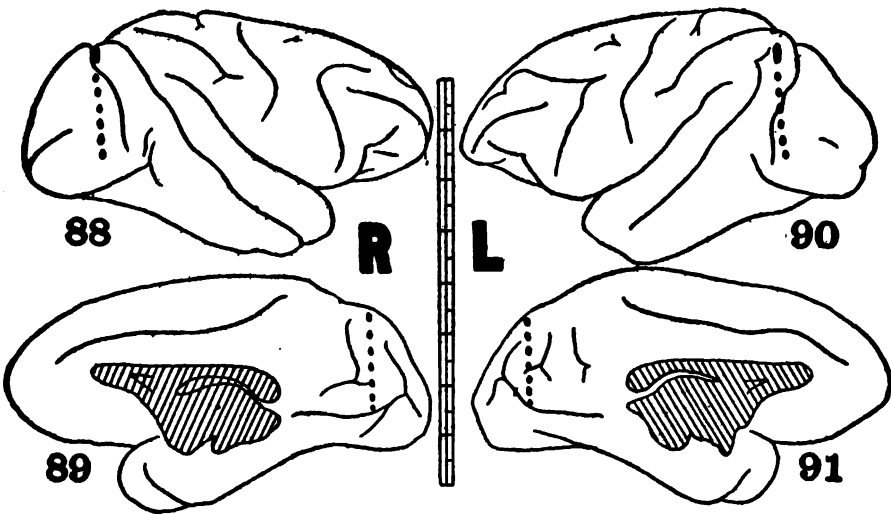
By the following day the dilation of pupils and the nystagmus (suspected) had disappeared. The general movements were more quickly performed, and the accuracy was the same as that following the operation. General tests with food were made, the

animal reacting in a normal manner, selecting pieces of food from a number placed on the floor of the cage, and rejecting those things which were of no value, and also those which it was known not to prefer, e. g., plain bread. No special tests of color were made on this day.

Two days after the operation the animal appeared able to see and to discriminate all kinds of food and other objects held in any part of the visual field, but when tested with the colored breads (which experiment was performed first on this day when the animal was hungry), the animal reached for and took both kinds of bread. It appeared at this time that the animal had lost the ability to discriminate the colors red and yellow in the bread test, although when tested with other foods of similar color (carrot, tomato, yellow turnip), there was no hesitation in selecting the red (tomato) and of selecting these vegetables instead of the red and yellow breads.

No special tests were made on the following day—the animal appearing to be in the same condition as that just mentioned. However, on the following day, four days after the operation, the animal was first tested with foods placed in different parts of the cage, with foods held in different visual segments and with foods outside the cage, the animal reacting at all times fairly quickly and accurately and at no time making errors in adjustment of more than one cm. After these tests (the monkey being not absolutely hungry) the tests with colored bread were made, and in the ten trials the monkey did not select the yellow bread once, and took only the red. It appears likely, therefore, that the errors made on the second day following the operation may have been due to hunger, any food being preferable to the animal than no food. The results of this test are particularly instructive, especially when they are considered in conjunction with the results of the similar tests with Monkey 7. With Monkey 7. it will be remembered (page 91), the animal discriminated the colored breads perfectly on the second day after the operation, but on the third day it appeared to have lost the ability. The latter result, in my opinion, is to be explained as due to the fact that these tests were performed at the beginning of the series of observations on that day, when the monkey had

not been fed for 24 hours, and when it was very hungry. A similar conclusion was drawn by me in regard to the monkey now under consideration. In the first tests of discrimination retention, the experiments were performed after a period of food abstinence, and the conclusion is drawn that the taking of both foods at this time was due to the extreme hunger of the animal. That this conclusion (or a similar one) is correct is evidenced by the fact that the discrimination of the breads on the fourth day following the operation was perfect from the first trial, and there was no mistake, not even that of touching the yellow pieces of bread. On the succeeding days the discrimination was perfect. The animal was killed eight days after the operation to determine the pathological condition.



FIGS. 88, 89, 90 and 91. Monkey 8. Lateral and mesial aspects of the hemispheres, showing the course of the knife cuts and indicating the approximate extents of the lesions. Made from photographs. Slightly reduced.

At the autopsy, the brain was found to be in a healthy condition. The positions of the insertion of the knife into the hemispheres were visible, and it appeared that the knife had separated most of the occipital (visuo-sensory) lobe from the anterior part of the brain. The appearance of the brain is shown in figs. 88 to

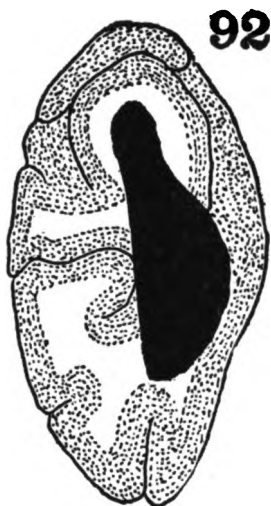
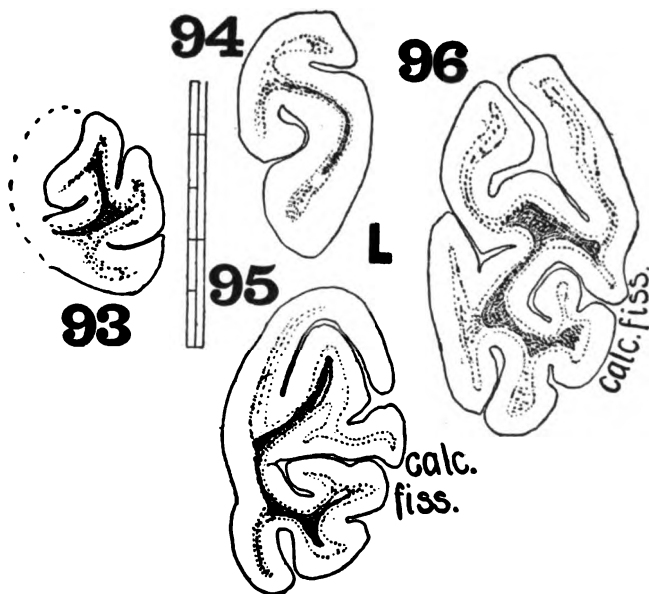


FIG. 92. Monkey 8. Gross appearance through part of one section of the left hemisphere. Free hand, slightly enlarged.



FIGS. 93, 94, 95 and 96. Monkey 8. Microscopical appearance of frontal sections, showing degenerations as indicated by the method of Marchi. On the right side only part of one section was preserved, which probably corresponds with section 95 on the left. Edinger drawing apparatus, slightly enlarged.

91, in which are indicated the general courses of the knife cuts. The brain weighed 107 grams.

The brain was prepared for examination by the Marchi method to show the fiber degenerations, and at the time of the autopsy a drawing was made of one of the gross sections on the left side, in which there appeared to be an extensive hemorrhage. This is shown in fig. 92. At the time of the autopsy the right side was cut in the same way as the left, but none of the sections showed the appearance indicated on the left. After hardening, however, each of the three sections on the right was again divided and an appearance similar to that on the left was found on this side. In gross section the hemorrhage on the right side appeared to be more extensive than that on the left, including practically all of the white matter. At this time no sketch was made of the extent of the lesion on the right side, and this is especially unfortunate on account of the later accidental destruction of most of the posterior portions of this hemisphere. In the process of hardening and infiltration, all of the pieces of the right hemispheres, with the exception of part of one, disintegrated in the celloidin, leaving a muddy residue and this part of the brain was, therefore, not examined histologically. The part of the right side which did not disintegrate probably corresponds with section 95 on the left side, although this is not certain because so much has been lost.

Following is the report of Dr. Lafora on the microscopical findings: "Monkey 8. Section of the white substance, Marchi method by Dr. Achúcarro. On the left side the degeneration is not very well marked because of the short time the animal was permitted to live after the operation (eight days). Many fat drops, however, are observed in the sections, and they are more abundant in the *radiatio optica*, and in the *fasculus longitudinalis inferior*. In the white substance near the cortex many fat drops were also observed. From the study of the degenerated fibers, it is not certain that all the optical fibers on this side have been destroyed, but that many, perhaps most, have been affected by the degeneration, there can be no doubt. The section on the right side shows about the same degree of degeneration as those on the left, and although none of the parts can be accurately dis-

tinguished, it appears that this section corresponds with 95 on the left side."

The report of Dr. Lafora indicates a marked destruction of fibers on the left side which corresponds closely with what is to be expected from the extent of the hemorrhages indicated in figure 92. It is especially unfortunate that in this case the hemorrhagic appearance on the right side was not drawn and that the pieces of this hemisphere disintegrated and prevented a careful histological examination. Sufficient is shown in the part of the right side to show the marked degree of degeneration and to indicate an amount of destruction of fibers (fatty degeneration) corresponding with that on the left.

DISCUSSION OF RESULTS

The observations recorded in the foregoing pages show that destruction of parts of the so-called visuo-sensory cortex is accompanied by derangements of a varied character, but of minor degree when compared with those recorded by most previous investigators. Only one animal showed the marked visual disturbances which are comparable with those which have been reported by Munk, by Ferrier, by Schäfer and by others, and in this case the amount of destruction was much greater than in any other animal, and the disturbance did not become marked for some days after the operation.

The effects of the operations on the animals may be grouped into four general classes: the general appearance of the eyes and the ability of the eyes to move, the fields of vision, the discrimination of objects and of colors, and the ability of visual-motor coördination. In no case were all of these elements affected, and in most of the animals, as has been noted, the amount of disturbance was slight and was often limited to one of the elements just enumerated.

Some of the animals showed pupillary disturbances immediately after the operation. Three (monkeys 1, 2, and 8) had dilated pupils, but it is not possible to say definitely whether this condition was due to the effects of the operation as such or to the anesthetic. It is well known that following operations on

other parts of the body there may be pupillary disturbances, and these have been taken to indicate the effects of the anesthetic. In these monkeys we are also justified in concluding that the dilation is the effect of the anesthetic, although in one case it must be remembered that the pupils were not equally dilated. In the case just mentioned the left pupil was decidedly larger than the right, and this may have been a sequel of the operation, although it is not possible to determine this with exactitude. The fact that the other four animals did not show pupillary disturbances would lead me to suppose that in the cases of monkeys 1, 2, and 8 we had to deal with a chance condition, not with one intimately associated with the destruction of the occipital cortex.

It may be different, however, with the other eye movements. Immediately after the operation monkey 2 showed a tremor of the eyelids, and this was not the effect of the anesthetic, but an effect undoubtedly due to the operation on the occipitals. After the second operation on monkey 1, I observed that its eyes were turned inwards and upwards, and after the operation on monkey 8 the eyes were directed toward the right. In addition, monkey 8 showed a nystagmus following the operation, this condition persisting for some time. It was also observed that at the time of the operation on monkey 7, the occipital lobe being cut in this operation, the right eye slowly rotated upwards and turned toward the right at the time the section was being made.

These oculomotor disturbances were noticeable in only four of the animals upon which operations were performed, and it can not be concluded that they have a direct bearing upon the problem of the functions of this area. It is known that stimulation of the occipital lobes produces movements of the eye balls and of the intrinsic eye muscles, and it is possible that the motor defects are due to the destruction of the mechanism which, by its activity, produces corresponding movements. That the defects persisted for only a few hours or a few days indicates however, that the phenomena are due to an irritation rather than to a paralyzing effect, and it is pertinent to consider the slow upward rotation of the eye in monkey 7 at the time of the section as evidence of this. This condition corresponds with what has been found by

some investigators upon stimulation of the occipital cortex, and since it corresponds also with the more lasting (although transitory) effects in monkeys 1 and 8, it is at present justifiable to conclude that it is an irritative effect.

The general statement may be made that the operations did not produce any alteration in the ability of the animals to discriminate objects, for in all cases the animals were able to select food and to pick over the floor of the cage for objects which were later conveyed to their mouths. In only one case was there any difficulty, and this animal did not show this difficulty immediately after the operation, but only after a week. That all the animals which had previously been trained to discriminate colored breads were able to discriminate them after the operation is evident from the records of the individual cases. It was unfortunate that the tests of color vision after the operation could not be performed in exactly the same way as they were performed before the operation, but the results which were obtained with the new methods speak plainly for the retention of the color discrimination. In most of the cases the color tests were delayed for at least two days following the operation, so that any general shock effects of the operation might pass away and so that the animal should be in as normal a condition as possible for the tests. Even with this amount of time it was sometimes found that the animal would not take the colored breads, and the tests had to be made on a succeeding day. Monkey 1 discriminated the colored breads two days after the first operation, and three days after the second operation. Monkey 2 discriminated the colored breads three days after the operation. Monkey 4 discriminated the colored breads two days after the operation; monkey 5 did not work well during the first few days after the operation, but showed color discrimination ability on the sixth day, and similarly with monkey 6. Monkey 8 discriminated the colored breads on the fourth day, and monkey 7 was tested on the same days as the operations (two of which were performed on this animal) and both times was able to select the proper colors. From these results it can not be doubted that the ability to discriminate colors remained after the extensive operations which were performed, and it appears plain that the parts of

the brain which were destroyed have not the function of color perception or of color discrimination.

In addition to the special color tests, general tests were made with each animal, and the results of these general tests are in agreement with the special tests of color which were made with the breads. Even immediately after the operation, if the animal was sufficiently recovered from the effects of the anesthetic, there remained a marked ability to discriminate colored foods, and in most cases the judgment of the animal was unerring. When cotton wool, rolled into balls, was introduced into the cages, the animals at times picked up this material and placed it in the mouth, but this is exactly what a normal monkey does, and the fact is not evidence of a lack of color discrimination or a lack of discrimination of different foods. The selection of the pieces of melon which in color closely resembled some of the green pieces of bread, but did not resemble them in texture (the visual quality of which is uncertain), the selection of tomato instead of the red bread, and the selection of different foods from each other, as shown particularly in the accounts of the actions of monkeys 3, 4, 6, and 8, all indicate that the visual discrimination of these animals remained approximately normal. That the discrimination ability was not alone that of color is shown by the results with monkey 3, which had been trained to discriminate different sizes. This animal, it will be recalled, retained this ability after the operation, and it can not be taken as evidence other than that for the retention of the general ability of visual discrimination.

It would have taken too long a time to detail for each animal all the observations which indicated the normal amount of discrimination ability, but in addition to the observations which have been recorded above, and others which have been contained in the individual case histories, it may be well to indicate a few other facts which were observed. Whenever the door of the observation cage was opened the animal immediately walked out. In only one case was this discrimination ability disturbed, in monkey 5, and this animal showed at the same time an almost total loss of visual ability. When the operated animal was introduced into a cage with a normal animal it immediately walked

over to the second animal, picked him or her over for dandruff, and acted exactly like a normal unoperated monkey. In some cases when a male operated animal had been introduced into a cage with a female normal animal, he immediately mounted the female for sexual congress. It was found possible to fool an animal once or twice with cotton wool, but not more than this number of times, and this fact indicates either the retention of the discrimination ability it previously had or the retention of the general ability to learn very rapidly to discriminate particular objects. Whichever conclusion is considered correct makes little difference, for both indicate a high degree of discrimination ability. When food wrapped in papers or small wads of paper were introduced into the cages, the operated animals did not place the covered food or the wads of paper into the mouth, but pulled away the paper from the food, or pulled apart the wad of paper. In both cases there was a discrimination of the paper from food, for it was the habit of the animals to place food immediately into the mouth. When water was placed in the cages, most of the animals immediately stooped down to the dish and drank. In this experiment there must have been the retention of a visuo-motor habit, comprised of visual discrimination of the dish and the movements of the body, or there must have been a very great ability to discriminate slight degrees of variation of visual intensities or qualities. The threatening of strangers, a common trick with these monkeys, was shown in almost every case, a fact which also leads to the conclusion that there was an ability to discriminate me from the other observers who came to see them after the operations.

Some of the facts which have been cited above may also be explained in another way, viz., that the discrimination took place not because of a visual discrimination ability, but because of a smell discrimination ability. This might apply to the threatening attitude and to the behavior with another animal, but it can not be stretched to include all the observations which have been recorded. On the other hand, it may be said that the smell component in monkey life is not as great as in the carnivora, and that these animals depend much more upon vision and upon hearing. Smell ability may be taken to explain the ability to

perform a certain isolated act, but it can scarcely be taken to explain the ability of the animal to select unerringly five pieces of melon or of pear from among a large number of other pieces of food, since the animal did not place its nose near the food but selected the food apparently by eye and hand.

The observations regarding the visual fields are with the exception of those on monkey 5 consistent. No constriction of the field was found immediately after the operation in monkeys 1 (after two operations), 2, 3, 4, 6, 7 (two operations), and 8. In some of these animals there was what might have been taken by a casual observer to be a constriction of the field, but in the two monkeys in which such a defect was noticed, it was shown by careful tests to be something different from a constriction of the field. Monkey 3 showed a condition which has been interpreted as a diminution in visual acuity, because the animal was able to select things in all fields but selected those in some of the fields more readily than those in others. Monkey 4 showed an inaccuracy in reaching for things held above and below the direct line, but here we are dealing with a complex condition rather than with a simple inability to see with a certain part of the retina. It will be remembered that monkey 6 showed a similar defect after six days, but that on the first few days after the operation there was nothing to indicate a variation in the field of vision.

The movement phenomena were similar in all the animals. The variations in movement are of two kinds, the time and the accuracy. After the first operation monkey 1 was slow and indecisive, and in grasping movements made errors of about 2.5 to 5 cm. Immediately after the second operation the animal was slow and awkward, the movements were deliberate and inaccurate, and in all these ways they were quite unlike those of a normal animal. Twenty-two hours after the operation on monkey 2, it was noted that the movements were inaccurate, the inaccuracy lasting for about three days. Monkey 3 made a number of unsuccessful attempts to grasp food which was presented to it, and for a number of days the adjustment was not like that of a normal animal. Monkey 4 was also inaccurate in the taking of food, and monkey 5 was more inaccurate than any of the other animals. The last named animal at first could reach food

within 30 cm. of its body with a fair degree of accuracy, but outside of this area the movements were slow and inaccurate. This animal eventually became blind, and the results must be considered apart from those of the other seven animals which retained their visual ability. All the other animals showed inaccuracies, slowness and apparent indecisions.

These movement disturbances were found only in the processes in which the eyes had to contribute. They were not found with the movements which were of a reflex type, they were not found for movements which did not require the coöperation of the eyes, and no paralyses or pareses of the eyes or of any of the body muscles could be determined.

At first glance it may appear that we are here dealing with a visual disturbance which is evidenced by the lack of adjustment between the visual sensations and the movements of the arms and hands in grasping, but a few words may be said upon this point. Simple movements, such as those of grasping for food, depend upon a number of factors, which for simplicity we may reduce to three—the sensory, the motor, and the associational. It is by the proper combination of these that movements are performed accurately and quickly. If by any means the connection is interfered with (in the diaschisis sense of von Monakow), the end result of the afferent stimulation shows an abnormality, which may vary from a slowness or an inaccuracy to an apparent motor loss. All movements depend upon two primary factors, the motor and the sensory. If one of the two primary factors be disturbed, the movement as a whole is affected. It makes no difference in a general way which of the two primary elements be affected, except that if the motor element be destroyed there results a total paralysis. With the incomplete destruction of the motor element and with the complete or incomplete destruction of the afferent or sensory element, the general effect may be the same, viz., an inaccuracy, a paresis, or a slowing in movement. In the case of the reflexes the elimination of the sensory element may even produce a complete loss, just as well as the total elimination of the motor element. In the present work, however, we have to deal with a motor disturbance, not with a motor loss, and it is apparent that the effects can not be considered off-hand

as purely motor, and on the other hand, we must not conclude too rapidly that the effects are purely visuo-sensory.

That the disturbances are not of a motor type is well shown by the fact that other movements of a similar character were not affected at the time of the operation. The movements of conveying food to the mouth were accurate and quick. They were exactly like those of a normal animal, as far as this could be determined, and there was no motor defect in any of the movements concerned with parts of the body other than those of the hands and arms. The movements of the head, of the eyes, and even those of the hands and arms, when the animal did not have to deal with the taking of food, and with other movements in which the visual apparatus was involved, were well executed, and were as quick as those of a normal animal. It was apparent that the motor disturbances were associated movement disturbances, and were those in which the eyes were involved.

Since the movements which were affected were those in which the eyes were concerned as one of the afferent factors, we must look to see whether or not this element (the eye) has been disturbed and has produced the motor derangement. We have, I think, the right to conclude that the skeletal muscles or the direct motor control of these muscles have not been disturbed, because movements which did not involve the visual element were executed well. On the other hand we also know that for the execution of an accurate movement the afferent elements are most important. Coördination and the production of accurate movements are brought about largely through the association of afferent impulses with the efferent, and in the coördination there are many afferent elements associated. In connection with the movements under discussion, the more important (possibly the sole) sensory elements are the visual (including the sensations or perception of eye movements) and the musculo-tactile. Several times in the case histories it was mentioned that animals which exhibited an incoördination showed this especially for movements of grasping objects at a distance from the body. When, for example, objects were rolled toward the animal or when the objects touched any part of the body, the animal made quick and accurate movements, with a degree of coördination

apparently normal. When, however, such objects touched the body and bounded away the objects were not accurately grasped, but the animal fumbled in the place on the floor where the food had been, and in many cases was not able to locate the object without great difficulty. At times, also, a monkey would give up trying to secure the food which in rolling had touched the body and had then bounded away, and it was evident that the actual coördination of movements which were made with the musculo-tactile element alone was not impaired but that the impairment was due to some other element. In the case of the animal which eventually became practically blind (monkey 5) although food which was held at a distance of about 10 to 15 cm. from the head of the animal was not accurately grasped, other food which touched the body or the body hairs was taken immediately and accurately. Observations similar to these were repeatedly made with all the animals, and in every case in which there was a marked degree of incoördination or of inaccuracy in adjustment when the food was held at a distance from the body, there was no inaccuracy and no incoördination when the food was held close to the body or when it was touching the body hairs.

We are justified in concluding, therefore, that the movement disturbances were due neither to a disturbance of the motor process (muscles and motor impulses) alone, nor to the lack of or to the disturbance in physiological connection between the motor impulses and the tactile and muscular sensations. In view of this conclusion we are forced to conclude that the disturbances were due to a nervous factor connected with the visual apparatus. Here there are two sensory elements which may have a part in the disturbance—the true visual and the movement. The retention of the ability of visual discrimination speaks against the view that the disturbance was due to a true visual disturbance, for we are unable to say that there has been in these cases any constant visual disturbance of the nature of an amblyopia, nor even of the nature of a lessened irritability (in the sense of Loeb). On the other hand, there is nothing distinctive in the results which absolutely excludes this factor, except that we have found none of the animals unable to distinguish dif-

ferent kinds of food which have had very similar color and light qualities. If this element were the important one, I should expect to find that the animals would not be able to distinguish one kind of food from another which has a similar color and a similar brightness. This discrimination of food was found to be intact in all animals, and the only observations which may be taken to negative this conclusion are those of Dr. Barnes (p. 86). With these conclusions of Dr. Barnes I do not agree, as I have indicated in the proper place. It appears to me unlikely that a lessened irritability (which must be shown by a lessened discrimination ability) should have been present in all the animals which showed the movement disturbances, for, as has been said, all the animals were able to distinguish one kind of food from another, and each animal made a selection of food when different kinds were presented simultaneously. If this factor be excluded, as I believe it should be from the evidence at hand, there remain only two elements which might have contributed to the disturbance. These elements are the sensations of the movements of accommodation (i. e., of the lens, etc.) in the focussing of the images on the retina, and the sensations arising from the movements of the extrinsic eye muscles. We have very indirect evidence of the part which the two afferent motor processes took in the disturbances in the monkeys. Observations were made of pupillary changes in some of the animals, and of pathological conditions of the extrinsic eye muscles, but none of the conditions was constant. We are unable to determine the sensory losses in these fields in animals, on account of the lack of information which the animal can give us, and on account of the inability to judge of the focussing and of accommodation abilities of the animal. The evidence is too general to exclude one of these factors and it appears to me that the only conclusion which, under the circumstances, is warranted is that the motor disturbances are due to the loss of or to defects in the sensational elements from the oculo-motor apparatus. That the eye ball movements and the movements of accommodation may remain normal even after the destruction of the corresponding cortical center is not unlikely, for we know that these movements are reflex, and are only affected in a general way by the activity of the cortex. If the corpora

quadrigemina remain intact the eye movements may remain almost normal, and they may be produced in such a way that the impression is obtained of complete and normal cortical control. This, I believe was the condition in the monkeys which I studied.

It is unfortunate that the work of Vitzou and that of Panici are not accompanied by illustrations of the extents of the lesions in the animals upon which they operated, for their physiological results have certain relations with the present work, which can not be discussed fully without the accurate description of the lesions. We do know enough, however, to be able to say that their results resemble those recorded in the foregoing paper to an extent unlike those of previous investigators with the possible exception of those of Ferrier. The accounts of Ferrier's monkeys with occipital lesions (see pp. 14-16) read much like those given by me and by the other observers who had an opportunity to study the animals carefully after the operations. It will be remembered that Ferrier's animals could see although the occipitals were extirpated, they ran away when they were approached, they avoided obstacles which were in their path, and one animal was able to see sufficiently well two hours after the operation to be able to pick up particles of food from the floor of the cage.

In view of the possibility of the establishment of new paths, possibly though lower centers, it is not surprising that Vitzou's animals were able to recover to a larger extent their visual ability, but it must not be understood that the recovery was complete, and in fact there is nothing in Vitzou's work to indicate the possibility of discrimination of a high degree. The fact that animals may relearn habits after the frontal lobes have been extirpated was interpreted by me to be due to the formation of new paths, and it appears to me most likely that Vitzou's animals also formed new paths for certain visuo-motor habits, even though the occipitals were destroyed.

The results obtained by Panici are most nearly like those recorded in the foregoing paper, but from the accounts of the animal activities it is impossible to determine how much of the visual functions, which he judged were retained, were retained because of the lower reflex connections, and how much real dis-

crimination ability was retained. The criticism of the work of Goltz applies here as well as it did to the original work, and it may be said again, as von Monakow has said, that the retention of simple eye movements, or even of simple visual-motor habits, etc., does not indicate whether or not there has been a retention of vision in the sense in which this term is properly used. There is undoubtedly a connection between the eyes and certain motor centers, but we are not able to say that discrimination has taken place, and without evidence of visual discrimination the interpretation that visual functions *per se* remain will not stand.

The consideration of the results recorded by me, with those which have been recorded by Ferrier, by Vitzou and by Panici, lead directly to the conclusion that previous investigators who have assigned to the occipitals a visual function have destroyed large portions of the lobe, and have been content to assign a visual function to its several parts as a whole without dividing it into individual elements. The conclusion is forced upon us that the lateral parts of the occipital lobes, which also have the calcarine type of cortex, have not a true visual function but a function in connection with the afferent impulses from the eye muscles. This, it will be noted, is not inconsistent with the results of previous investigators, although it is opposed to the conclusion that the whole occipital portion of the cerebrum is visual in function, and although it is opposed to the conclusion that in this portion there is a projection of the retina, such as Henschen believes.

SUMMARY AND CONCLUSIONS

The monkey readily learns to discriminate colors if the colors are parts of objects to which the animal attends.

Extirpation of the lateral parts of the occipital lobes does not interfere with color discrimination.

Extirpation of the lateral parts of the so-called visuo-sensory cortex in the monkey does not produce disturbances of a true visual character.

The destruction of the lateral parts of the occipital lobes is followed by disturbances in coördination of movements which are based upon the sensations from the eye and its appendages.

The disturbances in coördination are not due to a lack or to a loss of the true visual element, but to a lack or to a loss of the afferent motor elements, viz., those from the intrinsic or from the extrinsic eye muscles.

One animal in which the cortex surrounding the calcarine fissure was destroyed showed a true visual disturbance corresponding with those described in man.

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Association Tests

Being a Part of the Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests

By

R. S. WOODWORTH

Columbia University

and

FREDERIC LYMAN WELLS

MacLean Hospital

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ASSOCIATION TESTS

I. SCOPE OF THE WORK

The present paper forms part of the report of a Committee of the American Psychological Association, appointed in 1906, "to act as a general control committee on the subject of measurements." The Committee consists of Professor James R. Angell, chairman, and Professors Judd, Pillsbury, Seashore and Woodworth. This Committee was authorized to organize sub-committees and to secure the assistance of other members of the Association. A sub-committee on association tests was appointed, consisting of the present authors, and the present paper, the report of this sub-committee, is to be regarded as a supplement to the "Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests," published in 1910 as No. 53 of the *Psychological Monographs*.

The Association entrusted to its Committee two general lines of work: "first, the determination of a series of group and individual tests, with reference to practical application, and second, the determination of standard experiments of a more technical character." The sub-committee on association tests has confined itself to the first of these two lines of work. Leaving aside the more elaborate procedure, with chronoscope and lip key, we have fixed our attention on the "tests" so frequently employed in individual and pathological psychology for determining the speed and quality of association. Tests are needed which shall not require elaborate apparatus nor the expenditure of much time on the part of the individual tested. Many such tests are in use; these we have attempted to sift and, where possible, improve. The manner of giving the tests has varied from one experimenter to another; and we have attempted to ascertain the advantages and defects of the different procedures, and to make recommendations accordingly.

The efforts of a standardizing committee are likely to be regarded with disfavor and apprehension in many quarters, on the ground that the time is not yet ripe for stereotyping either the test material or the procedure. It may be felt that what is called for, in the present immature condition of individual psychology, is, the rather, free invention and the appearance of as many variants as possible. Let very many tests be tried, each new investigator introducing his own modification; and then, the worthless will gradually be eliminated and the fittest will survive. Admitting the general justice of this point of view, we still believe that work such as is here undertaken may be of service in two ways.

First, we hope that the tests herein recommended may find application where no special reason exists for the introduction of a new test. Often appeals for tests of proved value are heard from those who desire to study individual, race, sex, child or pathological psychology—from investigators who have not the time or inclination to devise new tests, and who, moreover, wish to be able to compare their results on one class of subjects with results already obtained on other classes. If every fresh student employs new tests, the incomparability of the results entails much wasted effort. Individual and class psychology is, almost of necessity, a cooperative enterprise. The advantages to be hoped from standardization are much the same here as in the field of anthropometry.

Second, it can scarcely fail to be true in psychology as in all other sciences that a full study of the *methods*, though too time-consuming and too remote from final results to be attractive at the start, is certain to lead to more reliable results in the long run. In the field of association—aside from the more technical experiments in memory—the methods have not been much subjected to the kind of experimental criticism which is here attempted. Usually the investigator has pressed forward to the solution of his problem, devising tests that seemed suitable to his purpose, and then abiding by them. Our concern being, on the contrary, exclusively with the tests themselves, we have sought for evidences of their relative value, relying at first on the ex-

perience of previous investigators, but in the last resort on renewed experiment with this end in view.

The tests which we have thus selected are in some degree analogous to "tested reagents" in chemistry. They make no claim, indeed, to be "chemically pure;" that is to say, they can not be guaranteed to give a true measure of every individual tested. Any mental test is sure to be vitiated in some cases either by peculiarities of an individual's training and information, or by the accidental variations to which mental performance is subject from moment to moment. These sources of error exist in all measurements of intellectual abilities. In the face of such difficulties, some investigators have felt it necessary to retreat from a quantitative attack on individual psychology; while others, more hopeful, have sought to neutralize the error of the single measurement by statistical methods. In the study of class differences, they have relied on averages from large groups; and in the study of correlations, they have endeavored to correct for the attenuation resulting from chance errors in the single measurements. But either reliance on the averages of large groups or reliance on Spearman's attenuation formulae is a reliance on probability, and therefore sure to be justified in the long run, but equally sure to be treacherous somewhere or other. Certainly, therefore, it is wise to eliminate from the tests all possible sources of error; though other sources of error still remain, yet for every defect eliminated there is an increase in the reliability of the individual measure, and so of the final result. Now most of the tests hitherto employed involve sources of error which can be eliminated once they are detected in practise. Many of these sources of error are little details in the construction of the tests; for example, one or two of the words selected as stimuli may have been ambiguous, or unfamiliar to many subjects. Our work has very largely consisted in attention to such details; and while we cannot hope to have attained perfection of detail, we are sure that we have taken some steps in that direction.

There is general agreement, in practise, as to what shall be included under the heading of association tests. There is the "free association" test, and the various tests of "controlled as-

sociation." In theory, indeed, it is difficult to draw any sharp line between association and memory, or intelligence, or reaction time. Every mental test involves association; but, in practise, the association test is regarded as limited to rather simple intellectual performances, and thus is distinguished from more complex tests of intelligence. On the other hand, the association between stimulus and response which comes into play in the simple reaction, or in the discriminative reaction, is simpler and quicker-acting than that of the "associative reaction." The line is, however, not sharply defined, and we have included in our list of tests one or two (as the "number-checking test") which might be more properly classed under the head of discrimination.

The distinction between association and memory experiments is even harder to draw in theory, though in practise the two are well enough distinguished. In the typical memory experiment new associations are first formed and later examined as to their strength; whereas the association experiment deals with association already formed, and does not control the process of their formation. Herein appears an obvious deficiency of the association experiment as compared with the standard experiments on memory. The memory experiment deals with a limited system of associations, formed specially for the purpose of the experiment and under controlled conditions. The association experiment dips into the general mass of the individual's associations, formed at various times and under varying conditions, with varying degrees of frequency, recency, vividness, emotional and intellectual value; and all these conditions vary from one individual to another. An experiment in the formation of entirely new associations gives all individuals an equal start; but a test dealing with previously formed associations can not hope to be perfectly fair. It aims, let us say, to give a measure of the speed of the individual's associative processes; but what it actually measures is, to a large extent, the familiarity of the particular associations called for, and the freedom of these associations from external interferences.

In the face of these difficulties, the association test may still prove of value. It may serve any one of at least three purposes,

and must be specifically adapted to the purpose which it is required to serve.

(1) A measure of the speed of formation of new associations. Such a test is indistinguishable from an experiment in memory or practise; but we have included one such, the "substitution test."

(2) Mental Diagnosis. Here the fact that the same association may have very different values in different individuals is fully recognized, and the object in view is to determine the value of a given association in the individual. Besides the emotional value, of which use is made in "psychoanalysis", the interest of a particular association may be the object of inquiry, as in "*Tatbestandsdiagnostik*." Also, the individual's familiarity with a certain sort of subject matter, or with a given form of logical relation, may be the thing measured. Thus the psychoanalytic viewpoint in association tests can be used, not only for the diagnosis of disturbing ideas and complexes, and for the detection of concealed knowledge, but also for showing the lines of thought with which an individual is conversant, and the sort of relationships along which his mind habitually moves. These uses of the association tests often require such close adaptation of the experimental material to the special object in view that they cannot easily be provided for by a standardized series of tests.

(3) A measure of mental alertness. The speed of an associative reaction depends not only on the strength of the associative tendency called into action—and thus on the previous training of that association—but also on the "determining tendency" or "adjustment" or "set of mind." In controlled association, the speed of the reaction depends on the efficiency of the control. In free association, also, a certain adjustment is required in order that the stimulus may call out a quick response: there must be a receptive attitude, a repression of any train of thought that would interfere with the speedy apprehension of the meaning of the stimulus; and there must also be an adjustment to give prompt expression to the first idea suggested by the stimulus. In a test of either free or controlled associations, calling for a series of responses in quick succession to a series of stimuli, the speed

of the performance depends on maintaining the proper adjustment throughout the series, in opposition to the many interfering tendencies generated by the successive stimuli. Periods of confusion are apt to occur in the course of such a series; and when they occur they impede the action of even well-trained associations. One cause of such periods of confusion, as has been abundantly pointed out by Jung and his school, lies in the emotional value of certain stimulus words; but that this is by no means the only cause of confusion is made evident in the color naming and similar tests, in which the same few stimuli are repeated many times in chance order. The associations required are here thoroughly familiar, and usually operate with great promptness; but at times they refuse to act properly, so that, in the midst of a series of rapid reactions, delayed and even false reactions occur to the same stimuli. The confusion here is sometimes due to wandering of the attention from the work in hand; but at other times it seems to be due to interferences generated by the performance itself. Whatever may be the cause of confusion in each particular case, efficiency in the test requires such a degree of control as will eliminate the confusion. Periods of confusion are but extreme manifestations of inefficient control; in a minor degree, the inefficiency of one individual in comparison with another is shown by uniform slowness of response.

In order, however, to make the association tests a measure of efficient mental control, it is necessary that the associations demanded shall be equally familiar to the individuals compared. In strictness, it is impossible to make sure of this; for the experimenter has no sufficient knowledge of the frequency, recency, etc., of the training which the several associations have received. The best that can be done is to call only for such associations as are familiar to all, or at least to the class of individuals to be tested.

Regarded as a measure of mental alertness or efficiency of control, the association test should be susceptible of standardization; and the efforts of the sub-committee have accordingly been mostly directed to this end. We have in every case but one—the Kent-Rosanoff experiment—sought for tests in which the

speed of association could properly be taken as the measure of efficiency—tests from which the question of the *quality* of the responses could be practically eliminated. To this end we have sought to determine, usually by experiment, what associations are so generally familiar as to be fair material for a test of individual differences in speed of association. We have also studied different methods of administering these tests, with a view to contributing towards uniformity of procedure; and we have, finally, endeavored to furnish average results obtained by these tests with one class of subjects, namely young adults of fair to good education.

During the progress of our work, several important contributions to the subject have appeared, of which two should be specially mentioned, those of Whipple¹ and of Whitley.² The aim and apparently also the method of Professor Whipple in preparing his lists of tests are the same as those of the present report; but the scope of his work is much more inclusive, and the present paper therefore represents a more intensive study of a limited field. The lists of tests here offered may be regarded as supplementing Whipple's list at a point where it is not especially full nor especially standardized.

Dr. Whitley's work is concerned very largely, though not exclusively, with association and similar tests; and her purpose is the same as ours, namely, to test the tests, and determine by experiment which are better and which worse. Her methods are however different from ours, in that, while we have been principally concerned with the details of each test, seeking to eliminate defects and sources of error, she has taken a large number of tests, as they stood, and compared the results obtained by their use. She has tried many similar tests on the same subjects, and has moreover repeated the same test a number of times, and then has evaluated the tests by the following criteria: (1) the better tests should not show rapid improvement with practise, for very rapid improvement indicates that some device for dealing with the test, or some adaptation to the conditions of the test, is of

¹ Manual of Mental and Physical Tests, Baltimore, 1910, pp. 254-270, 312-343.

² An Empirical Study of Certain Tests for Individual Differences. *Archives of Psychology*, No. 18, 1911.

prime importance; and as some subjects may chance to hit upon the adaptation or device at once, and others not, the first trial is likely to assign an individual a false position in the function designed to be measured; (2) the best test should show only a small variation in repeated trials (after the practise effect is allowed for), for the greater the variability, the less reliable is the single trial or the average of a few trials; (3) the best of a number of similar tests is that which correlates most closely with the average of them all, for this test represents the fairest sampling of the group of similar mental performances which it is desired to measure. In point of method, then, Dr. Whitley's work and ours are complementary; for a good test must both be free from minor defects, and must serve to indicate the efficiency of a function somewhat broader than that of dealing with the exact material used in the test. In regard to results, it is not easy to compare the two pieces of work, so much depends on the particular tests examined; but we find agreement at several points. Dr. Whitley, like ourselves, finds the use of written responses inadmissible in a test for speed of association; her results also tend to give the preference to the use of easy and simple material, such as we have adopted; and some of the tests which came out best in her comparison—such as an "easy opposites" test, a "first idea" test, a letter-checking and a form-checking test, and a form-naming test—are very similar to some included in our list.

II. QUESTIONS OF PROCEDURE

I. THE FORM OF RESPONSE.

Where the time of each single reaction is taken, as in the classical experiments on association time, the response has almost always been a spoken word, and the apparatus has measured the time to the beginning of the vocal utterance. But in tests which have measured the time, not of each single reaction, but of a continuous series of reactions, several forms of response have been used. Spoken words, written words, written letters, written Arabic numerals, and strokes of the pencil, checking or cancelling some of the (visual) stimuli, have all been used in different tests. In a test of the speed of any mental process, it is clear that the motor expression necessary for experimental purposes should require as little attention as possible and occupy as little time as possible. None of the above mentioned forms of response require much attention from an educated subject, but speech and cancellation have some advantage in this respect over writing. In respect to the time occupied by the movement, also, writing is at a disadvantage. The different times occupied by these various sorts of motor expression can be judged from the following results, obtained from two educated subjects:

Time for reading (either aloud or silently) a column	
of 20 disconnected letters or Arabic numerals..	6-7 sec.
Time for reading (either aloud or silently) a column	
of 20 short words, with a total of 22 syllables..	6-7½ sec.
Time for copying 20 one-place numbers.....	10-11 sec.
Time for copying 20 disconnected letters.....	12-13 sec.
Time for copying 20 short words, containing a total	
of 80 letters.....	27-35 sec.
Time for cancelling each of a list of 20 letters or	
words	6-8 sec.

The oral response, and the cancelling movement, have therefore a great advantage even over the writing of numerals.

So slow a process as the writing of words could never be thought of as a suitable form of response, were it not for the fact, that when a series of stimuli, such as a column of numbers to be added or a list of words to which synonyms are required, is presented together, the perceptive, associative and motor processes overlap; while the subject is writing the response to the first stimulus, he is already dealing with the second stimulus. If therefore the motor response is such as to occupy little time in comparison with the associative process, the overlapping brings it about that the time for the series of responses is nearly identical with the time of the associative processes involved; but if the motor response takes a much longer time than the associative process, the time of the series, because of overlapping, is nearly identical with the time of the motor processes. Overlapping causes a disappearance of motor time in the first case, and of the association time in the second case. So time-consuming a movement as writing can only be used as an index of the speed of association when the associations themselves are much more difficult and slow than those which are customary in mental tests.

With all this admitted, written responses might still find a defender, on the ground that the writing should be delayed by any halt in the associative process, so that, on the average, the longer the time required to write the list of responses, the slower must be the association. This is probably true; but it does little to weaken the objection to written responses. For, first, if only one individual is considered, or only individuals having the same speed of writing,—and if, also, the various words to be written are suitably adjusted as to length—then the longer writing time indicates the slower association, indeed, but the indication is far from sensitive, and fails altogether below a certain limit. Thus, for example, the associations involved in reading a list of words, and those involved in naming colors, are both too rapid to be measured by aid of written responses. The results of one well-trained subject may be given. To react to a series of 20 patches of color by speaking the names required 12 seconds; to read the 20 printed color-names required but 6 seconds; but to write the names, either in response to the colors or

in response to a list of the names, required in each case 28 seconds. Here written responses conceal very considerable difference in speed of association. Again, in case of the "opposites" test, a subject reacted to a list of twenty very familiar stimuli, by speaking the opposites, in 15 seconds; to a slightly less familiar set in 22 seconds; to a list of the response words, by reading them, in 6.5 seconds; but to copy the words from the list required 29 seconds; to write the responses to the easier set required 31 seconds, and to the harder set 30 seconds. Thus written responses entirely conceal the differences in speed of associations, provided only the association time is not over one second; and that even without regard to variations in the speed of writing. When however different individuals are to be compared, the speed of writing must be considered; and as this speed varies at least in the ratio of 2 to 1, even in educated adults, and as moreover, there is no close correlation (as we have found) between the speed of writing and the speed of association among educated subjects, it is clear, in conclusion, that conditions can scarcely be so favorable as to justify the use of written words as responses in any test of individual differences in speed of association.

The case is not quite so unfavorable with the writing of single letters or one-place numbers. For example, it is easier to respond to a letter by giving the following letter than by giving the preceding letter; and this difference appears in either oral or written responses. (One subject, 2 trials, list of 20 letters: Preceding letter: oral, 32 sec.; written, 35 sec. Following letter: oral, 20 sec.; written, 25 sec.) The writing of single letters or numerals is an admissible form of response when the association time is over a second—provided the individuals tested are accustomed to rapid writing.

2. MEASUREMENT OF ASSOCIATION TIME.

As already remarked, the purpose for which the present set of tests is designed excludes the use of elaborate apparatus and therefore of the chronoscope and lip key. The custom of many

students of association time, in clinical and similar work, is to employ the stop watch, starting the watch together with the spoken stimulus word, and stopping it on hearing the beginning of the response. This procedure would seem to include the experimenter's simple reaction time (probably 150-200 σ under the conditions) in the measured time. Moreover there is no guarantee that the watch is started precisely together with the giving of the stimulus; a degree of error must be expected here; and moreover, the fifth of a second of the stop watch is scarcely to be called a fine unit. In spite of these objections, the use of the stop watch appears to be justified in practise, especially since the variation in association time is so great that significant differences can usually be established even with a rough procedure.

Another procedure, much employed when the chronoscope can not be used, is to expose (visually) a whole list of stimuli, and to require the subject to react to these in succession and without delay between the separate reactions. The time is then taken, not for the single reactions, but for the whole series. As the time necessary for reacting to the whole series is usually at least 10 seconds, and often much greater, the deficiencies of the stop watch are not serious in this procedure. As indicated in the preceding section, when the motor reaction requires little time or attention, the overlapping of motor and central processes brings it about that the time of such a series of responses is essentially central time. If therefore the total time of the series of reactions be divided by the number of responses in the series, the quotient should give the average association time.

It would seem possible, indeed, that overlapping should accomplish more than this, and make the average association time, computed as just described, considerably less than that obtained with single stimuli. Cattell¹ found that a series of disconnected words could be read at a rate of 200 σ per word, whereas the reading time for an isolated word was 360 σ . But in even slightly more difficult reactions, such as naming presented colors, this shortening of the reaction time, when a series of stimuli is presented together, does not appear; but the average time comes out at

¹ Wundt's *Philos. Studien*, 1885, 2, 635.

from 600 to 1200 σ . The same holds good for such associations as are involved in the opposites test. We have tested five individuals with the same stimulus words, first singly, and then, several months later, in lists. Though the first of these tests should have made the responses somewhat more familiar, only one of the five subjects reacted more quickly to the words in lists than separately; two subjects reacted more slowly to the words in lists, and two showed no marked or consistent difference. On the whole, the average time as obtained by timing lists of associative responses is no less, and probably somewhat greater, than that obtained from separate reactions.

Some explanation is demanded by the failure of overlapping to hasten the reaction to a series of stimuli. The explanation is probably found in interferences generated in the course of a rapid series of associations. Many associative tendencies are partially aroused by each stimulus word, and when no interval elapses between the successive reactions, the tendencies generated by the earlier members of the series must be held in check in order to give free play to the associations required by the later stimuli. Irrelevant associations enter and tend to impede the progress of the reactions. Introspection makes this view seem probable, for often the subject is conscious that trains of thought, started by the earlier stimuli, must be repressed in order to do justice to the later stimuli. Sometimes the response made to a stimulus is not wholly satisfactory to the subject; sometimes a second response to the same stimulus is suggested immediately after the first has been spoken; sometimes an interesting idea or disturbing emotion is suggested by a stimulus or by the response made to a stimulus. All such interferences die away with the lapse of a few seconds between the stimuli; but are present in full force when no interval is allowed. Success in dealing rapidly with a series of unrelated stimuli requires a higher degree of control than success in dealing with isolated stimuli.

This interpretation of the list or serial test is borne out by the following experiment.¹ The subject had before him a list of

¹ We are indebted to Mr. Franklin B. Pedrick for collaboration in this experiment. It is intended to present elsewhere a fuller report bearing on the question of fatigue within brief periods of mental work.

20 stimulus words, to which he reacted in quickest possible succession; but the experimenter, instead of timing simply the whole list, took the time for each reaction, or, at least, the interval between each two successive reactions. This was accomplished by bringing side by side on a revolving drum a Jacquet chronograph marking fifths of seconds and an electromagnetic marker connected with a telegraph key on which rested the experimenter's finger. The experimenter pressed the key on exposing the list to view, and then on hearing the beginning of each successive response of the subject. Thus a record of the distribution of time through the series of responses was obtained, having an accuracy somewhat superior to that obtained ordinarily with the stop watch. The method can not be employed where the series of responses is very rapid and regular (as in naming colors), for then a rhythmic tendency dominates the experimenter's hand; but when the intervals between responses vary irregularly from 0.4 to 2 or more seconds, the method is perfectly feasible. Nine subjects were so tested, each reacting to 20 lists of 20 words. The instructions called for supraordinate, subordinate concepts, etc., the task remaining the same through each list of 20 stimulus words. (The experiment was at the same time designed to indicate the comparative difficulty of the stimulus words, and so to aid in selection of the best lists.)

In combining the results obtained from several lists and from several individuals, with the object of determining the general distribution of time throughout the list, difficulty arises from the inequal difficulty of the lists and from the unequal speed of the individuals. If the times for all the first reactions are simply averaged, and so for all the second reactions, etc., the general tendency is obscured by the extraneous variations so introduced. We therefore proceeded as follows: Taking one individual's performance in response to one list, we determined the distribution of time throughout this one list, by first determining the average time of these 20 reactions, and the average deviation of the reactions, and then expressing the time of each reaction as + or - (according as the time of this reaction was greater or less than the average time of the twenty) such and such a per cent

of the average deviation of the reactions in that list. For example, a mark of -50 meant that the time of a reaction was 50 per cent of the average deviation less than the average time for the list. The same process was repeated with each of the 20 lists; and the marks so obtained were averaged for each position within a list. Thus an average of + 50 for an individual in the first place meant that his first reaction occupied, on the average, 50 per cent of his average deviation more than his average time. The same process was repeated for each individual, and the individual marks were averaged. This procedure, then, eliminates the absolute times, and also the absolute variabilities, and gives an average picture of the relative distribution of time throughout the list of twenty. The net result, on the average of the nine subjects, is as follows:

Position in list.....	1	2	3	4	5	6	7	8	9	10
Av. mark.....	+35	-50	-38	-22	-7	-17	-25	-32	-16	-10
P. E. of Av.	15	6	4	3	4	6	6	4	5	7

Position in list.....	11	12	13	14	15	16	17	18	19	20
Av. mark.....	+14	+26	+2	+34	+20	+32	+18	+6	+22	-4
P. E. of Av.	6	6	4	4	8	5	8	5	6	4

If the speed of reaction were uniform through the list of twenty, the average mark should be close to 0 throughout; but this is not the fact. In spite of the considerable variations and the rather large P.E., there can be no doubt that reactions 2-9 tend to be quicker than the average, and reactions 11-19 slower than the average. There is a slight slackening in the speed of reaction throughout the list. The first and last reactions are exceptions to this rule; for the first is slow, and the last is more rapid than those which immediately precede it. In regard to the first, individual differences are here very great and characteristic; and a fairly strong negative correlation (Pearson $r = -0.69$) appears between the time of an individual for the whole list and his relative time for the first word. This correlation is seen in the accompanying table.

Individual	Time for list of 20	Relative time for first reaction
		in per cent. of av. deviation
H	25.0	+179
Br	28.2	+ 41
Pf	29.0	+103
Pd	36.4	+ 10
R	37.4	+ 64
Wl	39.0	— 58
Bn	39.4	— 36
E	39.8	— 21
Wi	47.0	+ 32
Average	35.6	+ 35

The individual who is relatively slowest in the first reaction reacts more rapidly to stimuli in series than to single stimuli, and in this respect is rather exceptional. Probably he manages the "overlapping" of the successive acts better than most individuals. Accordingly his relative slowness in the first reaction may be probably explained as due to the necessary absence of overlapping at the start.

The fact, however, that *the reaction to the first word of a list is on the average slower than the reaction to an isolated word* shows that something besides overlapping and its absence are in question. Sometimes a subject reported that, in glancing at the beginning of a list, his eye had caught the second word along with the first, and that he was busied with the reaction to the second as early as with that to the first. It even happened, occasionally, that the reaction to the second word was ready before that to the first. This form of interference, incidental as it is to overlapping, would of course slacken the reaction to the first stimulus.

Two influences operate in reacting to a list that are absent in reacting to a single stimulus: interference and overlapping. The latter tends to accelerate the reactions, the former to slacken them, as compared with a reaction to an isolated stimulus. Overlapping can not exert its accelerating effect upon the first reaction; and interference, also, would usually not become operative at the start, but special conditions, such as seeing the second word simultaneously with the first, may cause interference to be strongly evidenced at the very start.

Aside from the slowness and great variability of the first reaction, the most salient fact resulting from the above experiment is the quickness of reactions 2-10 as compared with reactions 11-19. Why should the speed decrease from the second reaction till near the close, and then increase again? "Fatigue" and "end-spurt" are the catch-words that readily occur to mind; but neither of them is specially explanatory. As for fatigue, so short a performance can hardly cause much fatigue of the genuine, metabolic sort. Interference seems to be a more probable conception. Each succeeding stimulus, and each reaction, tend to evoke associations that are of no service for the purpose of the test. These must be repressed; all their allurements brushed aside. A straight course must be steered in spite of many cross currents. As these deflecting tendencies continually accumulate with the addition of fresh stimuli and reactions, the likelihood of disturbance increases.

The increase in speed at the very close can probably be understood as incidental to overlapping; for, though overlapping leads on the whole to increase in speed, it does require, at every moment, a division of activity between two or more reactions. At the close, this division of activity ceases, and the last reaction receives the benefit of the overlapping without any of the incidental drawbacks such as were mentioned above in relation to the initial reaction.

The interest of the above experiment, in connection with the matter of tests, is the demonstration that the list test brings in factors—call them interference and overlapping, or call them fatigue, end-spurt, etc.—which are not present in reactions to isolated stimulus words. The list test reaches a more complicated mental performance and calls for a higher degree of control.

It was desired to see whether a shorter list would show the same time-curve as the list of twenty, and whether a list of ten words might not be essentially equivalent to ten separate stimuli. The experiment was of the same general character as above described, but was done in a rougher way. Instead of employing a rotating drum, the experimenter held the watch to his ear and with his pencil made wavy lines in time with the tick-

ing of the watch—the record so left resembling the trace of a tuning fork. At each reaction of the subject, the experimenter made a break in his time curve, and thus recorded the time of each single reaction. The accuracy of these times is about equal to that of ordinary stop watch readings. Meanwhile, by consulting his watch face at the end of the list, the experimenter had the time for the whole list. This double use of the watch can be recommended when list-tests are used, for the record of the single reaction, even if not highly accurate, is of value as showing how much of the time is lost in a few slow reactions, and as making possible the calculation of the median as well as the average time. Some practise is of course necessary before the experimenter can successfully use this device.

The results of this experiment were treated by the same statistical method as above described for the preceding experiment. Thirteen subjects served, each reacting to 13-24 lists, the total number of lists being 243. The average distribution of time in a list is shown in the following table, the explanation of which is the same as given on p. 15 for the preceding table.

Position in list	1	2	3	4	5	6	7	8	9	10
Av. mark	+179	-45	-33	-25	-11	-2	-19	-10	+3	-41
P. E. of Av.	12	7	4	6	3	7	7	4	5	7

It is quite possible that the very long relative time indicated for the first reaction is in part an artefact; but there is no doubt that the first reaction is slow, and that the last reaction is quicker than those that immediately precede it, just as was the case in the list of twenty. Further, there is a gradual increase of reaction time from the second to the ninth reaction. This slackening is less marked than in the list of twenty, but it is still present to a degree. It appears in the average results of 9 out of the 13 individuals; and the 4 who do not show a slackening show no progressive change in either direction. The conclusion is that the same factors are operative in the shorter as in the longer list, though not to as high a degree.

The results of the preceding experiments may well be compared with those of an experiment in which the stimulus words were

presented orally and separately. Five subjects were examined, each 16 times, with the same set of 20 words in different orders. The time-curve for these results presents an entirely different picture from that obtained in the preceding experiments. The first reaction is not slow, but on the average, one of the quickest; the last reaction is, on the average, one of the slowest, and yet there is no progressive slackening of the reactions from the first to the last, but the speed remains, on the whole, very uniform throughout. The difference in the time curve of the two modes of procedure is probably to be explained by reference to interferences: when the series of reactions is continuous, interferences tend to accumulate with the progress of the series, but when a brief interval of rest intervenes between the successive reactions the interferences tend to disappear. It may be concluded that the continuous reaction to a series of stimuli is a more complex process than the reaction to a single stimulus, and requires a higher grade of control. The two forms of test are not therefore equivalent, and each may be a good test; but, for a start, preference should be given to the simpler form, namely to the reaction to separate stimuli. Some associations, however, such as the naming of colors or other familiar objects, or the simplest arithmetical associations, are too rapid to be timed, singly, by the stop watch.

There is another advantage in the timing of single associative reactions over the timing of a series. The latter method gives indeed the average association time for the stimuli used, but (unless supplemented by some such device as employed above for getting the single times) it shows nothing of the distribution of the association times. In particular, since a series is likely to contain a few reactions much longer than the rest, the average time is apt to differ considerably from the median or the mode, and therefore not to be fully typical. The very slow reactions are usually due to rather special causes, and their great influence on the average is undesirable. The best procedure would seem to be that of timing the single reactions, and using the median as the typical measure.

Where a list-test, or continuous test, is employed, our experience leads us to favor a rather short list. A list of ten stimulus

words can be timed with sufficient accuracy, and it is freer, on the whole, from interferences of a disturbing character. In spite of this judgment in favor of shorter lists, we have presented lists of considerable length, partly to provide for use with a time limit—which is the procedure favored by Professor Thorndike, Dr. Whitley, and some other investigators of wide experience—and partly to provide a sufficient list of stimuli for separate reactions, when that is the method adopted. When the continuous method is used with an amount limit, it would be better to cut the lists in half and take two readings. Two short tests are better than one long one, because the average of the two is freer from the influence of momentary disturbances, and because it allows better for the effects of adaptation to the novel conditions of the test.

3. INSTRUCTIONS TO THE SUBJECT.

The necessity of uniform instructions has often been insisted upon, and sometimes the instructions have been reduced to a set formula, in order that all individuals tested, receiving the same instructions, may be treated alike. A set formula is, however, no guarantee that the subjects are treated alike, for some may not comprehend the formula. With a rigid form of instructions, the test becomes partly one of the individual's ability to understand the instructions, and only partly a test of the function exercised by the test material. It would be better to provide separate tests for ability to understand instructions, and eliminate this factor from other tests, so as to make each, as far as possible, a test of one function. Proper comprehension of the experiment by the subject must not be sacrificed to an ideal uniformity of instruction. It matters little by what method is attained the uniform *result* of the understanding of the experimental task. However, there can be little question that the best method for this result is that of "learning by doing," and that the subject should learn and demonstrate his capacity for the prescribed reactions by going through them. Instruction should proceed by description, illustration, and execution. The subject should first be told clearly the nature of the test; then if possible he should see

the operator perform some example of it, and finally he should execute samples of new material himself. None of the preliminary samples should duplicate the actual test material; nor should the preliminary trials be multiplied beyond what is necessary to insure the understanding of the test and the first strain of adaptation to it. As a rule it may be wise to allow the subject to make correct reactions to two samples before passing to the actual test.

Samples for use in instructing the subject should be prepared beforehand and lie ready to the experimenter's hand. For his convenience, it has seemed best to us to provide blanks containing samples of most of the tests—one blank containing samples for several tests.

The number-checking tests and the directions test—to be described later—require not oral but written responses, and their execution uses up the blank provided for each subject, and also the sample used by each subject. Since this is not the case with the remaining experiments, the instructional material of these two tests is best kept separate from that for the remainder. It is reproduced¹ on the accompanying page ("Instructional Material I"). For the two forms of the number-checking test, two lines of material, organized in the same way as for the actual test blank, are provided. For the directions test, there are three directions similar to those on its actual test blank. For the remainder of the tests, the same blank may be used for instructing an indefinite number of subjects ("Instructional Material II"). For each of the forms of addition test, successions of seven figures are supplied, the subject after verbal instruction and illustration reacting to these precisely as to the subsequent test material. There are three sample words for the opposites test, and also three samples each for the considerable number of tests of partially controlled association. The substitution test and the color naming test supply their own instructional material. The six words provided for the free association test are contained neither in the Kent-Rosanoff experiment nor in the supplementary thousand-word list.

¹ Here, and in some other cases later on, the exact type, etc., of the blank is not reproduced.

INSTRUCTIONAL MATERIAL I.

Number Checking Test.

Form A. 45879236017418605923596084231782130756494582763901
76084395121947250836364570129865283940172376941850

Form B. 215864 381592 826739 967814 371245 942861
876395 269517 712983 368459 326748 258647

Directions test:

1. Write any number larger than 16.
2. Add one more dot to the largest group
3. Put a cross over the angle that opens downwards ∨ ^

INSTRUCTIONAL MATERIAL II.

*Addition Test.**Kraepelin Form*

4
9
3
8
6
5
2

Constant Increment Form

32
47
21
53
39
28
65

Opposites Test

better

glad

straight

*Logical Relation Tests.**vb-obj*

cut
buy
bend

supraord

horse
Paris
potato

subord

flower
lake
game

pt-wh

roof
tail
Germany

wh-pt

wheel
Europe
brush

agt-act

train
frog
sun

act-agt

shines
howls
crawls

att-subst

cold
cheap
narrow

Mixed Relations Test.

Box—square

Orange—

Woman—husband

Man—

East—west

Day—

Penny—copper

Nail—

Asia—China

Europe—

Grain—sand

Drop—

Am—was

Have—

Free Association Test.

fox
apple
fork

cure
quick
grass

Even after thorough and apparently successful instructions, it will occasionally happen that the subject's reaction to the actual test material shows at once that he is on the wrong track. In such cases the test must be called off. But if the experimenter is provided with pairs of equivalent tests—as can always be accomplished in the present series, either from the provision of duplicate test blanks, or from the cutting of a long blank into two, as previously (p. 20) recommended—then the material of the first blank may be used for further instructional samples, and the actual test carried out with the equivalent blank.

There is some difficulty in bringing all subjects to an equality in their attitude towards the matter of speed. Occasionally it happens that an individual does not try for speed, but only for accuracy or distinction or even for an introspective study of his performance. Since the time of the performance is the important matter in utilizing the results, it is unfortunate when a good subject fails to make an effort for speed. We have seen the standing of an individual among his fellows completely changed, in the middle of a series of tests, by his being informed that his performance was slower than the average; his times were at once cut nearly in half, while his accuracy was not lessened. We recommend that the instructions include some such statement as the following: "The main thing that we are after is to see how rapidly your mind can act. You need not be afraid to put on speed, for the test is easy and you are not likely to make mistakes. Of course, you should keep on the right track and not make mistakes, and for every mistake you will be docked a little—about two per cent" (or one per cent if the whole of long blanks is used). The docking by two per cent is, of course, purely arbitrary; and it may be desirable with some classes of subjects to make a larger correction for errors; but our experience with these simple tests has not revealed the need of any corrections at all for errors. It is probably wise, however, to mention the possibility of errors at the beginning, and to have an understanding, at least with mature subjects, as to the degree of importance attached to them.

III. CANCELLATION TESTS—THE NUMBER-CHECKING TEST

Controlled association tests are, in method, somewhat analogous to choice reaction experiments. Like them, they may involve a certain response to every stimulus, according to a prearranged scheme of reaction (the B-method of Wundt), or they may involve one single reaction to a single kind of stimulus among a heterogeneous group of stimuli, the C-method of Wundt. Here the subject either reacts or does not, i. e. has the choice between movement and rest. The present test is the only representative of this method among the experiments to be described. The general idea is the presentation to the subject of a blank upon which are printed a large number of different letters, figures or designs, and requiring the recognition of each of a certain symbol to be indicated by marking. There are measured the speed and accuracy with which this is done. This experimental conception is not a novel one, having had its origin something over 15 years ago, and having, in various forms, played a part in a considerable number of subsequent investigations. In company with the diverse forms of material that are available for its performance, it has also borne a diversity of names; the most familiar single form is probably the so-called A-test, first mentioned by Cattell and Farrand, which has long played a part in the Columbia Freshman Tests as a measure of "rate of perception." As a rule, it seems desirable to know a test rather by an individually descriptive title than by an at best somewhat vaguely defined mental function with which it may be related. We thus offer the present attempt at the standardization of this method under the name of the "Number-checking Test;" prepared for special purposes in two forms.

Form A. As this is, so far as we know, the first time that a form made up exclusively of numerals has been offered for this purpose, it may not be amiss to give some account of the considerations which led to their adoption. The use of ordinary con-

textual material has its advantage in certain individual applications of the test, but is out of place in standard form, because the content of the text employed disturbs the attention of the subject to the experimental task, and disturbs it in different ways and in different degrees for different subjects, thereby violating the first principles upon which these tests are constructed. All this quite apart from its absolutely chaotic arrangement of the significant stimuli. Piped type obviates most of these difficulties, especially if methodically arranged; yet even here it is difficult to avoid vocable or other combinations of character that would have significance external to the test. Geometrical forms satisfy the conditions better, but here arises the difficulty of finding a proper number of distinct geometrical forms, small enough for the requisite purpose, and recognizable with sufficient readiness; for every effort must be made to obviate false reactions. These considerations seemed to point pretty definitely toward the use of Arabic numerals. They are as readily recognizable as the letters, and the chance of any specially suggestive collocation is infinitesimal in comparison with the letters.. The number of the symbols, 10, lends itself logically to the use of 100 of each symbol in a blank of 1000 symbols, which is as long as there is any necessity of making such a blank.

The general character of the blank being thus determined, the arrangement of the material took place as follows: Twenty lines of fifty symbols each, properly spaced and justified, provided when printed in the regular eight point type a printed space with suitable margins on the regular blank sheets of the experiments. The arrangement of each line is such that it contains five each of the symbols 1,2,3,4,5,6,7,8,9,0. Each successive fifth of the line (10 symbols) contains each of the ten different symbols once. This arrangement of itself obviates the occurrence of "runs" of two or more of the same symbol, except at the beginning and end of each ten, and it was not allowed to occur here. The first ten lines of the blank being completed in this way, the second ten lines were constructed by reversing this arrangement. This procedure assured the approximate equality of the two halves of the experiment as well as the uniformity in the distribution of all characters throughout.

51684923701275048693418902563717560892437869043125
 78051342692409761538320416796848126739053790866214
 85978461025182374960859324107693452086179316758402
 27396508149736150284047859621309315648724235679081
 42530179868860915472936748012564931207586127490538
 94703856216093827145781096435252794163801048237956
 09825617438354692017602137958421849570360952186743
 10462795380628439751274560389135078421695681924870
 86149230574517286309195683274086207354912473501869
 63217084957941503826563271840970683915248504312697
 796213405842519386079048172386562830514975948071236
 96810537421945370268047238659190368271547503294168
 07342918659612487053198306547215793482608359726401
 34768125906307594812485973120671029645388471652890
 65973284010836149725253469018754172839061265830749
 83509472168570213946521084763927451906836897108524
 18097653242784651390312695874048205163794180569372
 20485761397168025439670142395806947328152016487953
 41256809735093762184869751402883516790429624315087
 52134096873429806571736520981439684057210732948615

Special researches may call for different blanks prepared on the same principle as those submitted. A great number of blanks may be derived from the present one by methodically replacing each figure by some other, thus everywhere substituting 1 for 0, 2 for 1, etc. The test has many desirable features as a measure of fluctuations in continued work. Where it is used for this purpose a much longer blank would probably be desirable, in which the subject should check a greater variety of numbers, say all the odd numbers. The time for every line could be noted, or if greater precision were required, the time could be reported electrically by the graphic method.¹ The experiment should also be especially convenient in studies of interference, requiring a subject practised for one number to check different numbers, and the like.

With the above arrangement, similar as it is for all symbols, it makes no difference, so far as the arrangement is concerned, which one of the ten symbols the subject is instructed to check. The symbols do not however appear to be equally easy and their order in this respect should follow their order of distinctiveness; so far as is determined, 1 and 7 seem to be the easiest, next 0 and 4, next 2, 3, 5, 8, and the hardest 6, 9. In the present experiments the symbol 0 has uniformly been the one checked. Since some subjects may discover essential features in the arrangement of the blank while others do not, and also with a view to obviating omissions, the subject should be uniformly instructed that there are in each line five of the symbol they are to check. This reduces omissions to an insignificant minimum, if it does not obviate them altogether; and this advantage probably more than compensates for the occasional delays that result from missing the next symbol in succession and having to go back and search for it. In practise the procedure recommended seems the more desirable with the subjects so far employed.

After the subject has shown proper understanding of the experiment by correctly executing the two similarly arranged lines of the instructional material, he begins the execution of the test

¹ Cf. von Voss, Ueber die Schwankungen der geistigen Arbeitsleistung, *Ps. Arb.*, II, p. 300.

blank according to the directions already outlined. A pencil should be used for checking, and while it were easy to appear hypercritical in this respect, even in so minor detail as the character of the pencil, approximate uniformity should be sought for among all subjects between whom direct comparison is to be made. It should be one of moderate softness, not harder than the No. 2 grade, otherwise the markings may require more than the accustomed writing effort to make them properly distinct. The timing of the complete execution of the blank is by the stop-watch, and it is advisable to take the time for the halves also; unless the watch is provided with a split-stop, it is scarcely feasible to take the half-time closer than the nearest second.

Probably no other of the present tests shows such irrelevant differences in the manner in which different subjects execute it. The check-marks vary from light, almost haphazard strokes, to heavy, labored scorings. It is impossible, even were it desirable, to secure uniformity in these respects. In such matters the subject must adopt his own optimum method. While it is not clear that any special advantage accrues from this procedure, the same must apply to the habit of some subjects to check alternate lines backward. More significant data are probably obtained by merely noting whether the subject modifies the more natural behavior to the alternating directions, and if this is done immediately. A few subjects, having checked the two lines of the instructions blank, have shown a tendency to check only the first two lines of the experimental blank, and subjects should clearly understand that the *entire* blank is to be gone through. Half of the blank is however long enough.

On rare occasions a wrong symbol is checked; this seems to be more frequent in the later stages of practise. As with the omissions, the influence of such errors is due less to their *direct* distortion of the final time than to the fact that the subject is quite apt to observe them and be disturbed by their occurrence. On the basis of false reactions, the test has not shown, in the writers' hands, workable individual differences in the "accuracy" of performance. There are perfectly distinct differences in the time of performance, and there seems little reason, in the present test, for extending the scoring beyond this single factor.

In this respect, the range of normal performance in the test at the beginning of practise would seem to lie between 100 and 200 seconds, with an average of 133 for the whole blank. The subjects averaging this figure were 20 men and 20 women of a similar group. It is worthy of note that the women averaged distinctly faster than the men, 123 as against 145 seconds, and also somewhat less variable, the m. v.'s approximating 15 and 22. One of the writers has usually found the sex difference in variability in the other direction, and, as is brought out just below, the difference between this and the succeeding experiment was somewhat more marked in the women. In both groups the second half of the experiment averages slightly faster than the first half, though there are great variations in this respect.

The function with which the test is concerned is one rather susceptible to practise, as anyone may readily discover for himself. In a succeeding experiment the average times dropped to 116 and 134 for the women and men respectively, and the first half of the experiment averages a little faster in the women. Much of the significance of the experiment depends on the preservation of the individual relationships originally indicated. They are better preserved in the men, the orders corresponding within 13% for them as against 24% for the women, though this is largely due to two cases, one of whom rises fourteen places, the other dropping ten. The function is evidently one whose expression in the test can be distorted by incidental factors that are as yet very imperfectly understood, and the advisability of the greatest possible standardization of the experimental conditions and material is only further emphasized.

For subjects who at the outset evince a fairly definite quality of performance, practise does not tend to any great alteration of relative position. There seems to be less individual difference in susceptibility to practise than in the range of performance at the beginning of practise.

Form B. This variant is termed the "number-group checking test." It will be borne in mind that the conditions of the above discussed experiment have been frequently altered to require the checking of more than one symbol. The most familiar form of

this is the so-called *a-t test*, in which the subject, using a passage of connected prose, checks every word containing both the letters *a* and *t*. Other tests of a similar nature have been known by the names of the symbols checked. This is not the same as merely requiring the subject to check all of two or even more symbols, since the symbols have now to occur in a definite combination. The original blank was therefore not adapted to this purpose, and in order to meet in an analogous way the possible requirements of such a test, a special form was constructed for it.

This test contains all the combinations of nine digits taken six at a time. The number of such combinations $= 9 \cdot 8 \cdot 7 \cdot 6 \cdot 5 \cdot 4 / 1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 = 84$. Each single digit occurs ${}_8C_5 = 56$ times; each pair of digits ${}_7C_4 = 35$ times; each three, ${}_6C_3 = 20$ times; each four, ${}_5C_2 = 10$ times; and each five, ${}_4C_1 = 4$ times.

In preparing the test, a separate card was used for each of the 84 combinations, which were first written in the order of the digits, e. g., 134689, 125678, 245789, etc. Then all the 35 cards containing both of the digits 1 and 2 were separated out of the pack, and this pair of digits was assigned to the various possible positions in the group of six with approximately equal frequency. At the end of this operation, therefore, there were, as nearly as possible, equal numbers of groups arranged in the following ways (the dots indicating positions not yet filled):

12....	1.2...	1..2..	1...2.	1....2
.12...	.1.2..	.1..2.	.1...2	
..12..	..1.2.	..1..2		
...12.	...1.2			
....12				
21....	2.1...	2..1..	2...1.	2....1
.21...	.2.1..	.2..1.	.2...1	
..21..	..2.1.	..2..1		
...21.	...2.1			
....21				

Owing to the preliminary shuffling of the cards, the relation of these positions of the pair 1,2 to the remaining composition of the combinations was haphazard.

Next all of the cards containing the pair 1,3 were separated

out of the pack and shuffled, and the assignment of the pair 1,3 to its several possible positions with equal frequency was undertaken in the same manner as before. The operation of pure chance was somewhat limited by the previous assignment of the digits 1 and 2. The same operation was repeated with each pair of digits, 14, 15, 16, 17, 18, 19, 23, 24, 25, 26, 27, 28, 29, 34, 35, 36, 37, 38, 39, 45, 46, 47, 48, 49, 56, 57, 58, 59, 67, 68, 69, 78, 79, 89, in this order. As a matter of course, the operation of chance became more and more restricted as this work progressed; and it became more and more difficult and finally impossible to insure that the several positions of a pair should occur with equal frequency. All that could be hoped for was approximate equality of arrangement of the different pairs; and a review of the result, at the close of this operation, showed that approximate equality had been attained. Each pair of digits, therefore, will be found not far from once or twice in each of its 30 possible positions. Since it is believed that the chief application of this blank will be for checking the groups containing a given pair of digits, this approximate equality of arrangement of all the possible pairs is probably what is most needed. Some care was however taken also to avoid undue repetitions of the same arrangements of groups of three digits.

After the internal arrangement of each combination of six digits had thus been determined, the cards were again thoroughly shuffled in order to determine a chance order of the combinations. But the order was not left entirely to chance, for the immediate recurrence of the same pair of digits in the same position was avoided. Such immediate recurrences are likely to be noticed and remembered and so interfere with the repeated use of the blank with the same pair.

The result of all the operations so far was the obtaining of a series of the 84 combinations of the nine digits, taken six at a time, with approximate equality in position of each pair of digits, and approximately chance order of the combinations. Now it was desired to double the length of the blank, and in such a way that the second half of the blank should be equivalent to the first half. Each of the 84 combinations was therefore to be repeated in a

new permutation, and the equality in the arrangement of pairs was to be maintained. This could be accomplished, without need of going through all the operations involved in preparing the first half of the blank, by simply permuting each of the groups of the first half in the same way. Each of them was, in fact, permuted according to the following scheme: 2, 4, 6, 1, 3, 5. The second set of 84 groups thus obtained must have all the characters impressed on the original set, only with change of the absolute digits; and there can be no duplication between the two sets. By similar schemes of permutation, 720 sets in all could be obtained without duplication at any point.

After the above blank had been printed, it was checked up in every way to see whether it was according to specifications, and found to be correct. Since, in spite of the statistical equality between the halves, there might still be inequalities in practise, due to the element of chance entering into the arrangement, the halves were empirically compared, by taking the time for each half both for each single digit and for each pair. Our results are not, indeed, numerous enough to establish the precise equality of the halves, but they give no reason to suppose the halves different in any respect.

Were the nine digits of equal perceptibility, this blank would afford a large number of equivalent tests. But the digits are quite unequal in perceptibility; and it is therefore necessary to establish the relative difficulty of the several tests by trial. We have tried the following tests: (1) cancelling the groups containing each single digit, 9 different tests; (2) cancelling the groups containing each pair of digits, 36 different tests; (3) a few of the 84 possible tests in cancelling groups containing three assigned digits. The results, though not as extensive as could be wished, show much regularity and can probably be taken as indicating, approximately, the relative difficulty of the several tests.

(1) Cancelling of groups containing a specified single digit. In this, as in all the following results, one half of the blank was used at a time, and the time is given in seconds.

Seven subjects, previously untrained in this test, were tested

983642	168379	694517	253914	745682	158923	729648
426357	372159	754936	297835	627519	786531	731469
654173	947386	589761	134852	146237	194526	936425
837162	691324	814536	326175	368792	549826	572194
458671	971648	479612	495683	764295	817243	916328
275148	318495	635728	596873	982563	431289	381647
513978	182765	615832	851279	498136	356719	412789
197584	563792	748315	861395	421856	973124	125437
918654	846975	453867	281463	213956	651274	526987
397841	961872	248691	574389	532416	723964	473519
872351	327984	437528	864712	825916	682543	534169
923871	632791	765429	235849	672834	295481	349257
867314	462758	486592	198537	871596	164985	247153
963458	981374	156843	259671	762491	983587	579361
345962	941258	182653	561487	435781	179428	731825
672389	346521	427163	281937	672539	985273	956142
312876	853926	587436	296851	784623	875126	513647
934612	739548	843216	215367	916483	294378	768914
954178	371629	529817	436978	123874	957641	682917
719325	294736	639187	286415	593182	297568	145389
594231	389254	196235	825749	461289	378652	672841
349716	427395	138962	268794	524617	358472	319546
714932	759431	382145	853624	714529	635819	237465
649752	718254	596743	862934	851763	329418	495867

in cancelling each of the nine digits, the order of these nine tests being different with different subjects, so that any transferred practise effect from one digit to another is to a large extent equalized in the average of the seven subjects.

Digits	1	2	3	4	5	6	7	8	9
Av. time.....	43.5	63.0	59.9	53.7	61.4	70.9	54.2	57.4	65.1
A. D.	4.0	7.9	7.1	5.4	6.3	6.2	5.5	7.6	11.0
P. E.	1.3	2.6	2.3	1.7	2.0	2.0	1.8	2.4	3.4
Total range	37-56	53-81	46-74	42-63	48-78	62-82	39-67	43-69	50-78

One subject made eight trials with each digit, showing rather slight improvement after the second round. For trials 3-8, his times average as follows:

Digit:	1	2	3	4	5	6	7	8	9
Av.	31.0	45.5	40.1	38.7	42.9	50.8	36.1	37.6	44.4
A. D.	2.2	2.2	1.1	1.9	1.1	1.0	1.3	1.7	0.5
P. E.	0.7	0.7	0.4	0.6	0.4	0.3	0.4	0.6	0.2

Much the easiest digit to cancel is 1. It is easiest with every subject tested, and in every trial. If the times for the other digits are expressed as per cents of the time for the digit 1, the following are the relative times:

Digit	1	2	3	4	5	6	7	8	9
Relative time, av. of 7 unpractised subjects	100	145	138	123	141	163	125	132	150
Relative time, one practised subject...	100	147	130	125	138	164	116	121	143
Relative time, 3 subjects, 2nd trial....	100	156	131	129	130	163	121	125	141
Av. of above, with double weight allowed for first trial	100	148	134	125	138	163	122	128	146

The relative times in the three sets of results are in fairly close agreement, and the combination in the last line of the table can certainly be relied on within a few per cent. It is certain that 6 is the hardest digit to find, as 1 is the easiest. The important practical question is whether any digits are of nearly

equal difficulty, so as to be available for equivalent tests. Approximate equivalence is assured for the following pairs:

- 4 and 7
- 3 and 5
- 2 and 9

Further experience with the tests will probably show the need for slight corrections in treating these pairs as equivalent.

When only one pair of equivalent tests is desired, the easiest is probably the best, especially as our results show that errors and omissions are less frequent with the digits that give shortest times. Thus, the seven unpractised subjects whose times are reported above gave the following average number of errors (mostly omissions) per test:

In cancelling for the digit 1.....	0	errors
In cancelling for the digit 4, 7 or 8.....	$\frac{1}{2}$	"
In cancelling for the digit 3 or 5.....	1	"
In cancelling for the digit 2 or 9.....	$1\frac{1}{2}$	"
In cancelling for the digit 6.....	2	"

Since the time measure is of most value when errors are absent, the digit 1 is indicated as the best to use, except when there is need of an equivalent pair of tests; in that case, 4 and 7 are the best to use.

In regard to a correction for errors, our experience has not shown the need of one. Our subjects have not seemed to save time by omissions, but the time has been about the same either with no errors or with one or two or even three omissions. These subjects were, to be sure, serious and attentive; and it is likely that a more varied experience with the test would show the desirability of correcting for errors. We judge that the corrections should be small, and suggest the addition of 2 per cent. of the subject's time as penalty for each error or omission,¹ when one half of the blank is used; or 1 per cent. when the whole blank is used.

(2) Cancelling of groups containing a specified pair of

¹ An expeditious method of detecting errors is afforded by a key on transparent paper, to be laid over the blank. Whichever digit is used, the number of groups to be checked is 56 in each half of the blank.

digits. One subject has made six trials of each of the 36 tests of this sort; and another subject has made one trial of each. The results appear to have sufficient regularity to indicate the relative difficulty of the several pairs, and to show something regarding the mental process involved in this form of test.

The time occupied in checking a pair of digits is always longer than the time for checking either digit alone, but less than the sum of the times for checking the digits separately. For example, a subject takes 42 secs. to cancel the digit 4, and 48 secs. to cancel 9; to cancel groups containing both 4 and 9 takes him 64 secs., which is 71 per cent. of the sum of 42 and 48. The time for a pair is closely correlated with the sum of the times for the digits of the pair, and is usually equal to about 70 per cent. of this sum. The results are condensed into the following table.

RATIO, IN PER CENTS, OF THE TIME FOR CHECKING A PAIR OF DIGITS TO THE SUM OF THE TIMES FOR CHECKING THE DIGITS SEPARATELY				
Subject	Average	P. E.	A. D.	Range
J. W. T.....	67.3	0.4	3.1	60-75
R. S. W. first trial.....	72.2	0.6	4.5	62-82
R. S. W. after practise.....	72.5	0.4	3.0	66-79

Since this "ratio" is fairly uniform, it can be used, in connection with our previous table of times for checking single digits, to indicate the approximate times for checking pairs. Equivalent tests can be selected in this way; among the tests which appear from all our present results to be nearly equivalent, we recommend the following two pairs: 23 and 89. These have the advantage of not conflicting with the digits 4 and 7 recommended for use when single digits are to be cancelled. The time for either pair is about twice that for the single digit 1, or about one-and-a-half times that for the single digit 4 or 7.

It is possible, from comparison of the results of the two subjects in the above table, that there are genuine individual differences in the "ratio," i. e., in the speed of cancelling pairs as compared with the speed of cancelling single digits. Such differences may however, be merely the result of the relative degree of practise in the two sorts of test. If the subject has gone further in

his practise with pairs than in his practise with single digits, the ratio will evidently be small. If he has had some practise with single digits, but none with pairs, his first experience with a pair is likely to give a high ratio. Thus, subject R.S.W., after making two trials with each of the single digits, proceeded to try in succession each of the 36 pairs.

The average "ratio," for the successive quarters of this series, was as follows:

	Av.	A. D	P. E.
First quarter	76.5	3.4	1.0
Second quarter.....	71.9	4.3	1.2
Third quarter.....	72.6	2.8	0.8
Fourth quarter.....	67.9	3.2	0.9

He then resumed practise with the single digits, and afterwards returned to the pairs, obtaining then the average ratio of 72.5, as shown above. If practise is continued *pari passu* with single digits and with pairs, the ratio would probably remain in the neighborhood of 70 per cent. But at the very start, the time for a pair is likely to be about 77 per cent. of the sum of the times for the single digits; this is indicated also by less complete results from several other subjects.

The dependence of the "ratio" on practise has a bearing on the theory of this test. The fact that the ratio is high at the first experience in cancelling for a pair of digits shows that the detection of a pair of digits in a group is a specialized performance, not reducible to the acts of detecting the single digits. The detection of any specified pair of digits is no doubt a specialized performance, susceptible of very special training; this has indeed been shown in similar cases by Thorndike and Woodworth. But in the present series of tests, the pair of digits cancelled was changed with each new trial, so that the training visible in the lowering of the ratio from 77 per cent. to 72 or 70 per cent. is an example of transferred practise, and indicates that there is some element of skill common to the checking of all the pairs of digits.

Though the ratio varies within rather narrow limits and shows a comparatively small A.D. (as seen in the table on p. 36),

yet there is sufficient variation to make it possible that the ratio varies according to the nature, difficulty, etc., of the pair employed in the particular test. We have been unable to find any characteristic difference, however, according to the difficulty of the digits entering into the pair, except that, in case of subject R.S.W., the ratio is low for pairs containing the digit 1. The subject, after practise, gave the following ratio for pairs containing the several digits.

Pairs containing the digit	Average ratio	P. E.	A. D.
1.....	68.2	0.5	1.7
2.....	73.1	1.1	3.6
3.....	72.8	0.8	2.7
4.....	72.8	0.8	2.8
5.....	72.1	0.7	2.3
6.....	73.4	0.7	2.4
7.....	73.7	0.7	2.3
8.....	71.8	0.9	3.1
9.....	74.8	0.8	2.6

In general, the ratio seems not to depend on the digit; and the same negative result appears in case of the other subject, J. W. T., who moreover does not show anything characteristic of the digit 1. But the above results from subject R.S.W. show an unmistakeably lower ratio for pairs containing the digit 1. The distribution is fairly bimodal, the pairs containing 1 forming a group by themselves.

Some explanation of the low ratio for pairs containing the digit 1 is afforded by R.S.W.'s introspective account. It early occurred to him that a good device for cancelling groups containing a pair would be to look first for the easier digit of the pair, and thus to look for the harder digit only in the groups where the easier digit appeared. In practise, however, this device did not seem to him to work very well, except when the easier digit was 1; he tried to use the device also when the easier digit was 4, 7 or 8, but without subjective indications of success. When one of the digits was 1, groups containing it could be recognized in indirect vision, and thus many groups could be passed over altogether in direct vision. Subjectively, this method

of working required more effort but appeared successful. The objective records, as crystallized in the "ratio," show that the device was a success in the case of the digit 1.

Further consideration of this point may throw some light on the mental process involved in this test. If finding a pair were the same thing as finding the members of the pair, with no overlapping, the time for the pair would be the sum of the times for the digits composing the pair—instead of being, on the average, only 70 per cent of that sum. There must therefore be considerable overlapping or condensation. On the motor side, there is a possible condensation of the checking movement, but this is so quick and automatic anyway that abbreviating it has probably little to do with the shortening of the time. More strain is probably put on the eye movements when the speed of the work approaches its maximum (about 3 groups covered per second); but since this maximum is not approached, in our results so far, except by one subject in case of the single digit 1, the probability is that the demands made on the eye are well within its motor capacity. The difficulty of these tests is mainly perceptual, and the overlapping which is effective in finding pairs of digits must occur in the perceptive process.

If the device described above as adopted by one subject in finding pairs of digits—a device which has frequently been adopted by other subjects in similar tests—if this device represented the essentials of finding the pair quickly, then the following calculation should hold good. The subject looks first for only one digit, and where he finds it looks for the other one. The task of looking for the second digit would be necessary only in $\frac{2}{3}$ of the total number of groups in the blank (since the first digit, or any digit, is present in 56 out of the 84 groups). If therefore this plan were carried out systematically and without hitch, the time for checking a pair should be equal to the time for checking the first digit plus $\frac{2}{3}$ of the time necessary to check the second digit in the entire blank. For example, in checking the groups containing both 1 and 2, the time would be that needed to find the 1's (and this is 31 secs.) plus $\frac{2}{3}$ of the time necessary to go through the blank for 2, namely $\frac{2}{3}$ of 45.5 seconds, or 30.3 seconds; which added to

31.0 secs. gives 61.3 secs. as the calculated time for checking the pair 12. But the observed time is considerably less than this, namely 53 secs.; and this same discrepancy between the calculated and observed values obtains in every instance. The time for checking a pair is never as long as it would be if the above device were followed systematically.

This device does not therefore constitute the essential mechanism of checking a pair of digits. The device seemed to work well with one subject, in case one of the digits was 1; but its conscious use only reduced the "ratio" from 72.5 to 68.2, or 4 units, whereas something else reduced the average pair from the maximum of 100 to 72.5, or 27 points. There must accordingly be some form of overlapping of which the subject is not clearly conscious, but which is much more efficacious than the best devices which he consciously adopts. Introspection gives some hints at such an overlapping. Sometimes, indeed, a group is successively examined for the two digits composing the pair; but this is rather the exception. Often the presence of both digits is simultaneously recognized; and still oftener the absence of the pair is recognized without a clear consciousness of which of the two digits is lacking.

(3) Cancelling of groups containing three specified digits. Our data here are limited to 25 tests with one subject. The time occupied in this test is, again, closely correlated with the sum of the times for the three component digits, and is equal to about 70 per cent. of the sum of these times. Apparently the ratio is slightly lower for three digits than for a pair, for the subject, R.S.W., gives an average ratio of 68.3 per cent., with A.D. of 5.4, and P.E. of the average of 0.9. This average is thus probably lower than the average of 72.5 obtained by this subject with pairs.

When 1 is one of the three digits, the average ratio is 62.0, A.D. being 3.4 and P.E. 1.1. The subject adopted the same device as in pairs containing the digit 1, and the results here are confirmatory of what has previously been said.

For two approximately equivalent tests, we recommend cancelling for 146 and for 257. The time for each of these is about

2.5-3 times that for the single digit 1, or about 2-2.5 times that for the single digit 4 or 7.

Use of the number-checking blanks with laboratory classes. As suggested above (p. 27), the number-checking blanks could readily be adapted for experiments in continued mental work, interference, etc. One of the writers has used Form B, the "Number-group Blank," with success in an experiment in practise and "transfer." Half of the blank being used as the unit, the subject first checked the groups containing the digit 6, then took a practise series of ten units with the digit 7; then one unit again with 6; then another practise series with digit 4; then one unit with 7 and finally one more with 6. The two methods of studying "transfer," namely the "cross-section" method, and what may be called the "successive practise curve" method,¹ are combined in this experiment. The tests with digit 6 give cross-sections before and after practise with other digits; and since the digits 4 and 7 are equally hard to find, the practise curve with the one, following that with the other, should show the effects of the preceding practise. Transfer is pretty sure to be in evidence in each student's results; these need, to be sure, some correction from control experiments in which the cross-sections are taken without the intervening practice.²

¹ Introduced by Bair, "The Practice Curve", Psychol. Rev., Monogr. Suppl. No. 19, 1902.

² See W. F. Dearborn, *Psychol. Bulletin*, 1909, 6, 44.

IV. ADDITION TESTS

For rigidity of associative control, no experiments surpass those involving the simple arithmetical processes. In these a certain arithmetical task is visually presented to the subject, and efficiency is measured in terms of time and error. To provide an objective criterion of the performance the subject is required to speak or write the result. According to what has gone before, oral response is employed exclusively in the present experiments. One advantage of the written response is thus dispensed with, namely the permanency of the record, through which to check its accuracy. This difficulty is best obviated through providing the operator with a key upon which the correct reactions are noted. The operator follows the responses of the subject on the key and so keeps account of the data to be recorded.

Such experiments with arithmetical processes have an almost infinite range of difficulty, varying in practise from the simple addition of a pair of digits to the mental multiplication of three and even four place numbers. The chief advantage of the former is their freedom from errors; of the latter, the greater proportion of time spent in the essential work of the test. At first glance, one might consider that this same consideration, which leads to the substitution of oral for written response, should lead to the rejection of the easier and adoption of the more difficult experimental material. But it were very easy to press this advantage too far, especially in tests that are intended for anything like general employment. The more complex intellectual associations would result in the average individual in an impossible number of errors, if indeed they did not prove too much for his patience as well as his powers. A test not intended for limited application should not be one limiting the subjects who can respond to it; the tests to be described here, therefore, deal with the simpler arithmetical processes, regularly of addition, though the material prepared is adaptable in various ways.

- I. The particular form of addition test with which the most

work has been done, and whose properties with reference to the work curve are best understood, is that of the Kraepelinian *Rechenhefte*. This is a pamphlet of twenty-four pages, upon each of which are printed nine vertical columns of 32 single digits in apparently random succession. It is possible to experiment with this material by continuously adding the successive digits, and announcing the sum total at stated points. The disadvantages of this procedure are very numerous and do not call for consideration here. A decidedly preferable method is the simple addition of the successive pairs of digits. That is, the first four figures in the *Rechenheft* being 8, 3, 5, 7 the sums announced by the subject are 11, 8 and 12. The subject continues to announce the sum of every figure plus the one next below it. Precise control of the whole process, both as to accuracy and time, is thus secured. As before mentioned, the operator checks the correctness of the sums, notes errors, and the amount performed within specified times. The usual periods of work with the Kraepelinian test have been of five and ten minutes each, and it has also been customary to record the amount of work done during the single minutes. The subject should not, as has been done, be called upon to make the records; all such tasks devolve properly upon the experimenter. It does not appear that a significant portion of the time is consumed in the motor process of response. At the beginning of practise, the number of additions made in five minutes is usually under two hundred. It need scarcely be said that the sums themselves could be read in a much shorter time; maximum speed of reading aloud in normal individuals averages not far from 100 words in 30 seconds.

Used in the above way the Kraepelin *Rechenheft* contains 31 additions per column, 279 per page. While the unpractised subject is not likely to do more than this in five minutes, a little practise will soon take him over the page, and it may be considered always advisable to open the *Rechenheft* to two full pages, (it should be held open with a clip, amply providing for any common performance without turning a leaf. One of our subjects, after prolonged practise, occasionally reached a figure above 558 additions in the five minute period. There is uniformly a con-

siderable practise effect in the test, perhaps due partly to the unfamiliar sort of reaction required to the material, but, as with the number-checking test, the individual differences in susceptibility to practise are small in comparison to the differences in the amount of work performed.

The Kraepelin *Rechenheft* is practically the only form of this experiment that permits long continued tests of the same subject with sufficient uniformity of experimental material; but as the present tests are not designed as practise experiments, it was thought advisable to construct a more convenient blank for a single or small number of determinations. It was also endeavored to improve on certain minor features of the *Rechenhefte*, as the odd number of columns and additions in each column.

The blank, reproduced on the page opposite, contains 24 columns of 26 digits (25 additions),—in all, 600 additions. This is probably beyond the 5-minute capacity of the normal subject at the limit of practise. The columns are arranged in six groups of four, each thus containing 100 additions with 104 digits. The scheme of the distribution of the digits was simpler than in the number-checking test. 99 slips were prepared, eleven for each of the nine digits. The five remaining slips bore the digit 5. Random drawings were made from the group of slips, and the digits were written in the columns in the order in which they were drawn. Each set of four columns (100 additions) is therefore a unit in itself, and is made up of a proportionate number of each of the nine digits in random order. The six series of 100 additions may then be considered as homogeneous and of approximately equal difficulty.

Subjects are not apt to comprehend this test so readily as they do, for example, the number-checking test. They may tend to add the numbers continuously, or more especially to add discrete numbers, thus with the column beginning as before, 8, 3, 5, 7, to announce the sums 11 and 12, omitting to add the 3 and 5. The operator must be well assured from the instructional material that the test is properly comprehended. Some subjects prefer to follow the columns with the finger; this should be permitted, on the same principle that permits reversed directions in the number-

checking test. The addition of the final digit of one column to the beginning one of the next is not required, nor do subjects ordinarily tend to do it, even though not specially instructed.

The test may of course be made of any length within the limits of the blank, but, from experience with the *Rechenhefte* one hundred additions appear sufficient for a unitary determination. The length of the present blank is believed to provide amply for sufficient variation of the experimental material for all ordinary purposes outside of special research upon the individual function.

The test requires the constant attention of the operator to the key to check the proper performance of the work by the subject. False reactions are more frequent than in the number-checking test, and they should be kept track of, though the writer has not seen an instance where they obscured individual differences in efficiency. The subject may not notice the error; if he does, the purpose should be to get him over it with as little disturbance and distraction as possible, and he should therefore be allowed to correct it or not, whatever is the path of least resistance for him. We believe this to be the sounder experimental practise, whatever might be said of it from an ethical standpoint.

2. In order to furnish a regulated experimental material which should have a greater flexibility of application than is usual in this class of tests, a second form of procedure is submitted, known as the *constant increment* test. This is a little-recognized method, but one which in direct comparison has shown superiority over other forms. It consists in presenting to the subject a series of numbers, requiring the identical arithmetical operation to be performed upon each. In the observations made with this test, the usual procedure has been the addition of 4. In this particular instance, there is perhaps no reason why the Kraepelin blank should not serve this purpose as well as that for which it is ordinarily used. In order however, to make the material adaptable also to subtraction, especially of larger figures, it is thought wise to preserve the special blank originally adapted to this test.

This blank contains 100 two place numbers. The unit places in these numbers are ten each of the figures 1,2,3,4,5,6,7,8,9,0. In

64	72	47	30
49	35	43	56
62	51	35	44
57	30	64	31
68	56	49	37
74	44	67	60
53	36	28	71
67	73	46	48
25	63	55	53
40	47	65	61
61	43	70	36
71	66	41	42
33	69	62	34
38	37	25	39
28	39	40	33
65	32	57	73
41	59	26	38
50	31	68	63
42	60	66	58
58	48	27	32
52	54	51	59
70	46	69	52
26	55	29	45
34	27	74	72
45	29	50	54

the tens places are ten 2's, ten 7's and twenty each of the intervening 3,4,5 and 6. These features are symmetrically distributed with reference to the halves. The subject speaks the proper responses according to the assigned experimental task; thus in the addition of 4 the responses begin 68, 53, 66, etc. It is probably advisable to confine the unit of observation to half of the blank, and even better would probably be two tests of one column each. Errors seem to be but slightly more frequent than in the simple addition of a pair of numbers; in their treatment, the same considerations obtain as in the previous form of test. Since the operation to be performed with the given numerals may vary indefinitely, no key is provided, but the experimenter may readily provide one himself for his own particular requirements, and should always do so; its employment being the same as in the Kraepelin form of addition test.

Results. Individual differences, due in part, no doubt, to differences of training, are very great in even the simplest arithmetical tests. Thus, while one of the authors has usually obtained, with the Kraepelin form of test, times of from two to three minutes for 100 additions, the other of us, working on 7 college and university students, has the following results:

Average time for 100 additions.....	107.2 seconds
A. D.	24.4 seconds
Range	65-164 seconds

With the constant increment test, the following results have been obtained from 10 subjects of the same class as above: Only one column was used in each test, and the times given are times for one column.

Problem	Add 4	Subtract 4	Add 17
Av.	33.9	41.1	97.4
A. D.	5.8	11.0	23.6
Range	24-49	25-67	62-158
Av. errors per column of 25..	0.3	0.2	2.4

Experience with other subjects leads one to suspect that the time per column, for adding 4, will often run up above 60 seconds.

V. NAMING TESTS

1. *The Color Naming Test.* No doubt the best-trained and quickest of all associative responses appear in the reading of words, letters and numbers. (See above p. 9). Next to this, probably, stands the naming of familiar objects. The color-naming test belongs here, and may be expected to give shorter times, per reaction, than any of the other tests included in the present selection.

The test blank shows 100 patches of color (besides 5 sample patches), each 1 cm. square and separated by spaces of 1 cm. from its neighbors. The colors are to be named in order, as in reading, and one side is indicated as the top. The arrangement is such, however, that the blank can be equally well used in any one of the four positions.

Care is needed in the selection of colors for such a test; for the color names required should be of universal familiarity, and there are few such names. White, black, gray, red, yellow, green, blue, brown, and possibly pink and purple are in sufficiently general use for our purpose. It seemed safer to exclude pink and purple; if white is then used for the background, seven colors remain; but it seemed better, on the whole, to employ only five stimulus colors, repeating each twenty times. After some experimenting, it was decided to use red, yellow, green, blue and black.

The use of the Hering red and green seemed undesirable, since they do not suggest the color names so promptly as do a red and green each somewhat nearer to yellow. To attempt to equate the colors in brightness would not be best, since this would mean toning down the yellow to a point at which it would be likely not to be called by that name. The blue must be pale in order to be distinguished from black as quickly as the other colors are distinguished from each other.

In the arrangement of the color stimuli on the blank, all sequences of the same color were avoided, as such direct sequences

enable the subject to combine two reactions, practically, into one. Too frequent recurrence of the same sequence of different colors was also avoided, the object being to compel a separate reaction to each single stimulus. It was desired, indeed, to have the different possible sequences of two and of three colors occur with equal frequency; but something less than perfection in this regard had to be accepted, for it was desired also to make the arrangement equally good for each of four positions of the blank; and all the conditions of perfection could not be met in both the horizontal and vertical lines at the same time. An incredible amount of time was consumed in arranging the colors to meet these simple requirements. The blank seems now to be free from serious blemishes of arrangement, and requires approximately the same time for reading in each of the four positions.

Preparatory to the test, the experimenter lays the blank before the subject, with only the sample line of 5 colors showing. The subject is directed to give the names of the sample colors; when he understands the task and knows what names to use, the whole blank is exposed at the word "Go!" The time for the first half, as well as for the whole blank, may well be taken. Repeated trials can be made with the blank in four positions. The line of 5 sample colors is to be omitted in the actual test. As usual, one half of the blank is long enough for a satisfactory test. A key of the series of correct responses will facilitate the experimenter's task.

2. *The Form Naming Test.* The blank next to be described under the head of the Substitution Test is similar in all respects to the color sheet, except that five geometrical figures take the place of the five colors. This blank can be used in the same manner as the color blank, the numbers written in the key line being disregarded.

Results. Here, as in most of the other tests, the results now available are insufficient to do more than give a general impression of the time required. In this case, too, the results are all from one class of subjects, namely college and graduate students. The whole blank of 100 stimuli was reacted to, and the time taken for the half as well as for the whole.

Color naming test.

		1st half	2nd half	Whole
9 men	Av.	30.6	35.1	64.6
	A. D.	3.0	5.2	7.4
	P. E.	0.8	1.5	2.0
5 women	Av.	26.4	29.0	55.4
	A. D.	4.5	3.6	8.1
	P. E.	1.7	1.3	3.0
Total range, both sexes together.....		22-41	24-48	48-89

Form naming test.

		1st half	2nd half	Whole
6 men	Av.	46.7	47.2	93.8
	A. D.	8.6	7.2	15.5
	P. E.	2.9	2.4	5.2
4 women	Av.	38.5	42.3	80.8
	A. D.	6.0	10.0	16.0
	P. E.	2.5	4.2	6.7
Total range, both sexes together....		31-60	29-58	60-117

From these data, it may be inferred (1) with reasonable assurance, that the color naming test is easier than the form-naming test. Comparison of the figures for the present color-naming test with those reported by Wissler¹ from the same class of subjects with the use of the Columbia color-naming test, which employs ten colors, makes it probable that the present test with five colors is noticeably easier—as was, indeed, intended.

(2) It may be inferred from the above table, with much probability, that a sex difference exists in the case of the color naming test, women being on the average quicker than men. This is the more probable because Wissler² obtained the same sex difference from much more extensive data.

(3) It seems also probable that the same sex difference exists in the case of the form-naming test. If so, the sex difference here in question is not specially related to the color sense, but rather to linguistic facility. The authors have in mind the accumulation of sufficient data to determine whether the appearance here shown corresponds to a real sex difference.

(4) In the color-naming test it seems probable, and in the

¹ *Psychol. Rev.*, Monograph Suppl. No. 16, 1901.

² *Op. cit.*

form-naming test, possible, that there is some slackening of reactions, such as was shown above (p. 15) to occur in other forms of test. On reference to the individual records, we find that 10 of the 14 subjects in the color-naming test took longer for the second half than for the first half of the test; and in the form naming test, 6 out of 10 did the same. Only one of the exceptions is more than a bare exception. The behavior of the subjects during the test shows periods of hesitation and obstruction, and even of false reaction—rather a strange phenomenon, in view of the great familiarity of the names and their correct use immediately before. The subject is aware of this inhibition, and it is a strange experience for him. The “mechanism” of inhibition can not here have the elaborate Freudian character; and in fact the experiment seems a good one to show the reality of other forms of inhibition in the recall of names. The real mechanism here may very well be the mutual interference of the five names, all of which, from immediately preceding use, are “on the tip of the tongue”, all equally ready and therefore likely to get in one another’s way. These periods of inhibition do not appear at the very beginning of the test, but most often, to judge from incidental observation, along in the middle. Some subjects, after succumbing a few times to interference, appear to collect themselves and do the last part of the test better than the middle. We have the records of the successive rows of ten stimuli each, in the case of five subjects in each test. The average time in seconds for each row is as follows.

Row	1	2	3	4	5	6	7	8	9	10
Color naming ...	6.2	5.6	5.6	6.6	6.8	7.3	7.7	6.9	6.3	6.2
Form naming ...	8.0	9.1	9.0	10.3	10.8	8.2	10.5	9.9	10.3	9.0

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VI. FORMATION OF NEW ASSOCIATIONS

The Substitution Test. This blank is modelled after one by Professor J. E. Lough,¹ but is simplified, in that only five (instead of twenty) different stimuli are used; at the same time, by employing geometrical forms in place of the letters of Professor Lough, it is partly freed from the danger that some subjects may hit upon easy mnemonics.

Since the names of the forms may enter into the subject's procedure, the forms should have equally familiar names. They should also be of such shapes that the blank, like the color-naming blank, may be capable of use in different positions. Only about five geometrical forms meet these conditions: the circle, square, triangle, star and cross. The blank is made up of these five forms, each repeated twenty times. The arrangement of the stimuli follows the same rule here as in the color-naming test.

At the top of the blank appears a line containing each of the five forms once, with a number on each. This line being covered, the rest of the blank is exposed to the subject, and it is explained to him that he is to write on each of these forms a number,—the same number as he will find on that form in the key at the top. In this test, the general rules of instructing the subject by aid of examples can not be exactly followed; for the association to be employed in the test should not be formed before the outset of the test itself, since this is a test of the formation of associations. When the experimenter is sure that the subject understands what is to be done, he uncovers the key, at the word "Go!" Besides taking the time for the whole and half, the experimenter may be able to get the times for each successive line, and so obtain a curve of the formation of the associations.

A misunderstanding which has occasionally appeared in the use of this test should be guarded against in the instructions to the subject. Some subjects have started to go through the blank numbering only one of the forms at a time, intending to

¹ Described by Kirkpatrick, *Studies in Development and Learning, Arch. of Psychol.*, 1909, No. 12, p. 36.

go through again for each of the other forms. It should be made clear that the forms are to be numbered in order as in reading or writing.

Results. Eleven educated adults (6 men, 5 women) gave the following average time in seconds:

	1st half	2nd half	Whole
Av.	79.6	65.1	144.7
A. D.	9.0	7.8	12.5
P. E.	2.3	2.0	3.1
Range	58-94	53-83	111-177

The gain from the first half to the second is perhaps not so great as would have been expected. In fact, few if any of the subjects fully mastered the key in the course of the 100 reactions.

Time was taken, in this test, for each successive row of ten forms, with the following average results (11 subjects):

Row	1	2	3	4	5	6	7	8	9	10
Av.	15.1	17.7	16.4	15.6	14.8	13.8	13.0	13.3	12.9	12.1
A. D.	3.7	2.8	2.8	1.6	1.6	2.3	2.2	1.9	2.4	1.9

The longer time for the first line than for the second is found in 9 of the 11 individuals; and 3 subjects do the first line as quickly as any other. It is, in short, possible to do the first line, by simple use of the key, in 9-11 seconds, and this is as rapid as any of the subjects became during the course of the test. Some subjects do the first line by mere copying from the key; others start to memorize and take longer on this line; this is probably the cause of the extra large variability for the first line.

The test is not long enough to permit the complete establishment of the associations; several blanks may be used in succession, and oral may be substituted for written responses in order to simplify the motor part of the performance. At the best, however, progress is rather slow; and, indeed, one would not expect these freshly formed associations to surpass readily the familiar associations involved in the form-naming test (p. 51), the times in which are, after all, not very much shorter than those in the last rows of the substitution test.

By use of a second key, the blank can be used for the study of interference; one of the authors has so used it in laboratory classes, following the general arrangement of Bergström's card-sorting experiment.¹ The blank can also be used for a simple cancellation test, similar to the number-checking tests.

¹ *Amer. Journ. of Psychol.*, 1893, 5, 356.

VII. LOGICAL RELATIONS

The form of test in which the stimulus is a word and the response another word standing in some assigned logical relation to the stimulus has been long and widely used, and has an intellectual atmosphere that makes it seem likely to prove a test of individual differences of the intellectual sort. At the same time, it is distinctly a test of the command of language, and when the measurement concerns the speed of the response, familiarity with the necessary words is a prime necessity. Unless great care is used in the selection of stimulus words, long reaction times will occur from the need of searching for the proper response words, and the test thus becomes predominantly linguistic in nature. Linguistic it must always remain to a considerable extent, no matter how much care is taken in the selection of the stimuli; but the effort should be to minimize the linguistic factor by selecting only stimulus words that are universally familiar.

Besides the familiarity of the associations employed, the test calls for skill in the handling of these associations; and it is this skill, most of all, which the test designs to measure. In other words, it is the efficiency of the "determining tendency", or adjustment to react according to instructions, which should be revealed by the speed of performance. The more completely this adjustment dominates the performance, facilitating the right responses and inhibiting other, interfering associations and perseverations, the less hesitation and confusion will occur and the more prompt will be the reaction.

In order to afford sufficient opportunity for the determining tendency or adjustment to show its efficiency, it is customary and evidently desirable to provide a number of stimuli in succession, requiring the same sort of response to each. There should therefore be a list of stimulus words for each of the logical relations along which the reactions are to be required. Thus the task of providing material for these tests consists in discovering a sufficient number of stimulus words of the requisite familiarity.

Only by actual trial can the suitability of the stimuli be ascertained. A word of apparently eminent fitness may prove to be unfamiliar to many subjects. For example, the word "false" seemed likely to be a good stimulus when the required response was a word of opposite meaning; but in practise much hesitation and uncertainty of reaction appeared in the responses to this word (instead of "true", some women subjects said "natural"). A word which seems perfectly familiar to the investigator may be unfamiliar to many subjects, and a word which seems perfectly unambiguous may convey an unexpected meaning to some subjects. To avoid all such difficulties with all subjects is too much to hope; but the test material should be freed from words that cause difficulty to a large share of the subjects. This is necessary at any rate if the time is to be taken, not for each separate response, but only for the whole series of responses to the list of stimuli; for otherwise the total time may be determined mostly by the difficulty of one or two of the reactions. Even if the times of the separate reactions are taken, the average time will suffer in the same way as the total time in the preceding case. The median time is mostly free from this source of error. But even so, lists of nearly uniform difficulty would form the best and fairest test material.

Our procedure in selecting stimuli for this class of tests was to start by getting together as large a number of stimuli as possible; to eliminate at once all that seemed ambiguous or unduly difficult and to try the remainder with a few subjects, timing the separate reactions, and eliminating the stimuli that gave the slowest reactions or that proved to be ambiguous or complex-arousing. The abbreviated list was tried in the same way with other subjects, and more words eliminated, till finally it appeared that the easiest possible list of stimuli had been secured. Unless it proved possible to secure a list of twenty easy stimuli, that particular test was abandoned. Thus, it seemed impossible to prepare a list of twenty words sufficiently easy for a synonyms test, except indeed for well-educated individuals. On the other hand, it appeared possible to select two lists of twenty for the opposites test.

The number of subjects employed in reaching the selection of stimulus words was greater in case of the opposites test than in the others, and this test may properly be regarded as more highly "standardized" than any other belonging under the head of logical relations. In the case of opposites, a long list of words was tried with 6 subjects, and the forty words selected from this trial were tried with 40 other subjects; some need of revision was then apparent, and a few more words were substituted from tests of a few subjects; then the revised collection of 40 was tried with thirteen fresh subjects, and a few minor corrections still introduced, which left the lists in their present condition. In the other tests, two to three times the desired number of stimulus words were tried with nine subjects, and the resulting selection was tried with thirteen fresh subjects; some minor changes were then introduced and the lists left in their present condition. The "mixed relations" test was selected gradually on the basis of results from fourteen subjects.

After the selection of the stimulus words came the question of their arrangement within the list. This matter of order of stimuli is not of great importance if the time is to be taken for the separate responses; but whenever the time is taken only for the series, the order of stimuli is a matter of some consequence. We recommend, it may be remembered, that the time be taken for the first half of the list as well as for the whole list, and even that the halves be given as separate tests; it is therefore important to have halves of equal difficulty. Moreover, many investigators find it convenient to allow a fixed time for each test, and to measure the number of responses that can be given in this time ("time limit method"); with this procedure it is important that the list shall be of uniform difficulty throughout, so that the number completed shall be a fair measure of the work done. Whatever be the procedure in giving the test, the most desirable arrangement of the stimulus words would be such as to distribute the difficulties evenly throughout the list. If it were really possible to discover twenty stimulus words of equal difficulty, the question of their arrangement would not arise; but this is not possible, for though the twenty stimuli be all decidedly easy, yet the reaction time to one will be two or three times as long as to

another of the twenty, on the average of as many as ten subjects. Since it is impossible to prepare a list of twenty stimuli of equal difficulty, we combined the words in pairs so that the pairs should be of equal difficulty, as judged by the sum of the reaction times to the two members of each pair. One pair thus may consist of the hardest and the easiest word in the list, and another pair of two words of medium difficulty; but the sum of the reaction times for the first pair is equal to that for the second pair, as judged from the records already in hand. (The pairs can not be hoped to be equal for all subjects.) These equal pairs can then be arranged in such a way as to avoid "constellations" or undesirable collocations of any sort; and the difficulties of the list will be pretty evenly distributed.

Two other points were considered in arranging the order within the lists. When the test is given with a time limit, it is especially desirable to have the responses of uniform difficulty in that part of the list where most of the subjects will be stopped, so that there, at least, the single words shall constitute equal units. We have therefore placed those of our "pairs" which are composed of words of medium difficulty in the midst of the list, from about the 8th to about the 16th word. If then the time limit is so chosen that the great majority of subjects shall be stopped in this part of the list, the separate words may, without much error on the average, be counted as equal units.

The other point concerns the writing of responses. In reality, as explained in the introduction, an easy association test is very ill adapted for written responses, because the time of writing is much greater than that of easy association, and individual differences in speed of writing altogether mask the differences in speed of association. However, in case of the opposites test, we have determined the writing times for the correct response and so distributed the stimuli that the writing times for the two lists of twenty, for the halves of each list, and, as nearly as possible, for the pairs throughout each half, shall be equal. This has not been attempted for the other tests given below, because the response words are not wholly determined in advance.

1. *The Opposites test.* This test has one advantage over all the others in the series of logical-relation tests, namely that the

answers can be definitely scored as right or wrong. Opposites apparently are the most available material for a test of completely controlled association—with the exception, indeed, of the naming tests and of the arithmetical tests already brought forward. For this reason, we have taken unusual pains with the selection and arrangement of material for this test. As it appears possible to select forty words free from difficulty, we offer two lists of twenty, with the object of making it possible to give two equally difficult tests of the same function. Since, however, it may be desired in some instances to have the very simplest material, we also present a list of twenty “Easiest Opposites”, all of which are included in the two lists of twenty.

The lists are printed on three separate slips,¹ in 12-point type, well-ledged. Lists I. and II. are of equal difficulty, and the halves of each list are equivalent, as far as can be judged from the results of their use so far.

The instructions, enforced by samples (see p—), require the subject to respond to each stimulus word by the word having the opposite meaning; as, “long-short”.

OPPOSITES TEST

<i>I</i>	<i>II</i>	<i>Easiest</i>
long	north	high
soft	sour	summer
white	out	out
far	weak	white
up	good	slow
smooth	after	yes
early	above	above
dead	sick	north
hot	slow	top
asleep	large	wet
lost	rich	good
wet	dark	rich
high	front	up
dirty	love	front
east	tall	long
day	open	hot
yes	summer	east
wrong	new	day
empty	come	big
top	male	love

¹ The type, etc., of these tests is not reproduced here.

2. *The verb-object test.* More good stimulus words are available here than in any similar test except that for opposites. Here again we have selected two equivalent lists, and also a list containing the very easiest stimuli, as judged from results with about 20 subjects. The verbs are to be treated as transitive, and objects supplied; for example, "sing song", "build house." The audible repetition by the subject of the stimulus word is not required, and may interfere somewhat with the experimenter's record; but it does not change the times to an appreciable extent.

VERB-OBJECT TEST

<i>I</i>	<i>II</i>	<i>Easiest</i>
sing	read	wash
build	tear	sing
wear	throw	bake
shoot	paint	read
scold	mail	chew
win	light	learn
answer	sail	mail
weave	spin	sweep
wink	lock	scold
mend	wash	wear
pump	bake	sharpen
learn	spill	kiss
open	kiss	smoke
eat	polish	answer
climb	sweep	climb
lend	fill	lock
smoke	sharpen	throw
singe	write	sail
dig	chew	dig
sift	drive	wink

3. *The supraordinate concept or species-genus test.* The instructions are to name a class to which the given object belongs, or to "tell what sort of thing each is"; as "oak—tree."

4. *The subordinate concept or genus-species test.* The instructions are to name an example of the class mentioned, or to "mention a—", as, "color—red".

5. *The part-whole test.* The instructions are to name the whole thing of which the part is mentioned; as "elbow—arm".

<i>Supraordinate Concept Test</i>	<i>Subordinate Concept Test</i>	<i>Part-Whole Test</i>
oak	color	elbow
measles	holiday	hinge
July	fish	page
shark	tool	finger
quinine	metal	wing
beef	vegetable	morning
canoe	coin	blade
banana	city	mattress
Atlantic	insect	chimney
Alps	food	cent
penny	fruit	sleeve
dictionary	disease	brick
cabbage	grain	deck
Rhine	drink	France
murder	month	pint
dog	ocean	fin
sparrow	language	steeple
London	river	month
football	newspaper	hub
rose	tree	chin

<i>Whole-Part Test</i>	<i>Agent-Action Test</i>	<i>Action-Agent Test</i>	<i>Attribute- Substance Test</i>
apple	baby	gallops	sharp
clock	fire	bites	hot
knife	dog	boils	dusty
book	laborer	sleeps	raw
hat	pencil	floats	deep
pencil	army	growls	ripe
hand	heart	sails	funny
dog	pin	roars	tall
oyster	gun	scratches	stormy
church	eyes	stings	new
chair	bird	shoots	hilly
bird	wind	melts	strong
banana	lungs	swims	muddy
shoe	bell	explodes	pretty
train	musician	aches	noisy
finger	parrot	blows	white
house	clock	mews	steep
coat	axe	cuts	round
cart	broom	flies	smoky
face	mosquito	burns	curly

6. *The whole-part test.* The instructions are to name a part of each thing mentioned; as "apple—core".

7. *The agent-action or subject-verb test.* The instructions are to put an appropriate verb to each noun as subject; or to "tell what each of these does or can do;" as "baby—cries".

8. *The action-agent or verb-subject test.* The instructions are to supply a subject to each verb, or to "tell what does or can do each of these things;" as, "horse gallops."

9. *The attribute-substance or adjective-noun test.* The instructions are to supply an appropriate noun for each adjective, or to "tell something that is or may be each of the following", or to complete the expression, "A good—", etc.; as, "sharp knife",

10. *The mixed relations test.* In the preceding tests, the task remains the same through a series of reactions; in the present test the particular relation along which the reaction is required to occur changes with each reaction—the object being to get some insight into flexibility of mental performance. We were long at a loss for some means of indicating the new task without lengthy explanations at each new stimulus and also without the use of such technical terms as supraordinate, etc. Finally a device used by one of us previously in the study of consciousness of relations seemed to meet our present needs: the relation along which the reaction is to take place is indicated before each new stimulus word by a pair of words serving as a sample. The subject is to note the relation of the second word to the first, and then find a word standing in this same relation to the third word. Thus, in the example "Box—square Orange—?" "square" gives a quality of "box", or, more specifically, the shape of the box, and it is required to mention the shape of an orange; in the example "East—west Day—?" since east and west are opposites the task is to find the opposite of day; and in the example, "Penny—copper Nail—?" the task is to mention the material of which the nail is composed. Some of the relations are not readily named, but little difficulty has appeared, with the adult educated subjects already tested, in grasping the relation from the sample given. Instructions for this test must proceed largely by

the use of samples, of which several must be given, in order that the subject may realize that it is not always the same relation that is needed, but a new relation each time as indicated by the first two words in the line.

*Mixed Relations Test**I*

Eye—see	Ear—
Monday—Tuesday	April—
Do—did	See—
Bird—sings	Dog—
Hour—minute	Minute—
Straw—hat	Leather—
Cloud—rain	Sun—
Hammer—tool	Dictionary—
Uncle—aunt	Brother—
Dog—puppy	Cat—
Little—less	Much—
Wash—face	Sweep—
House—room	Book—
Sky—blue	Grass—
Swim—water	Fly—
Once—one	Twice—
Cat—fur	Bird—
Pan—tin	Table—
Buy—sell	Come—
Oyster—shell	Banana—

*Mixed Relations Test**II*

Good—bad	Long—
Eagle—bird	Shark—
Eat—bread	Drink—
Fruit—orange	Vegetable—
Sit—chair	Sleep—
Double—two	Triple—
England—London	France—
Chew—teeth	Smell—
Pen—write	Knife—
Water—wet	Fire—
He—him	She—
Boat—water	Train—
Crawl—snake	Swim—
Horse—colt	Cow—
Nose—face	Toe—
Bad—worse	Good—
Hungry—food	Thirsty—
Hat—head	Glove—
Ship—captain	Army—
Man—woman	Boy—

Results with the logical relations test. After the tests had reached practically their present condition, they were tried with thirteen college and graduate students (in a few cases, the number of individuals was less than this). In these experiments, lists of ten stimulus were presented visually, but the time of the single reactions was roughly taken by the device mentioned on page 17. Usually two, and in the case of the opposites and verb-object tests four lists of ten were used, and each subject's average time per single reaction was obtained. The averages given in the accompanying table are the average of the individual averages, and the A.D. is that of the individual averages from the general average.

	Av. per single reaction	P.E.	A. D. of indivs. from general av.	Range of indivs.
Opposites 1 and 2.....	1.23	.06	.16	1.03—1.50
Opposites, easiest	1.11	.04	.12	0.85—1.40
Verb-obj. 1 and 2.....	1.39	.05	.19	1.08—1.75
Verb-obj., easiest	1.31	.05	.14	1.10—1.55
Suproord. concept.....	1.54	.07	.31	0.90—2.20
Subord. concept.....	1.84	.07	.31	1.20—2.63
Part-whole	1.53	.06	.27	1.03—2.50
Whole-part	1.57	.07	.32	1.13—2.35
Agent-action	1.30	.03	.12	0.93—1.70
Action-agent	1.55	.07	.32	1.03—2.68
Attrib.-subst.	1.53	.07	.28	1.08—3.05
Mixed relations	3.14	.13	.53	2.33—4.40

The degree of agreement between the results of the several logical relations tests is a matter of some interest as indicating to what extent a single test is a fair indication of the individual's ability in this whole class of performances. By methods which will be more fully described in another paper, we have determined the average standing of each of our thirteen subjects in the nine logical relations tests (excluding the mixed relations test), and have correlated this average standing with the standing in each single test. The results follow, in the form of Pearson coefficients, uncorrected for attenuation.

	<i>r</i>	P.E.
Correlation of Average with: Opposites.....	+.88	.03
Verb-object.....	+.70	.08
Subordinate conc.	+.72	.07
Supraordinate concept....	+.91	.03
Part-whole	+.86	.04
Whole-part	+.76	.06
Agent-action	+.83	.04
Action-agent	+.84	.04
Attribute-substance	+.54	.12

As far as these few results indicate, then, the opposites and supraordinate concept tests seem slightly better than the rest as representative of this general sort of controlled association. The correlation between the opposites and the supraordinate concept tests was +.70, with P.E. of .08, while the average correlation between any two of the nine logical relations tests is +.57.

*Comparative speed of the different forms of controlled
association*

It may be of interest, since results are available in several tests from comparable and highly reliable (though not numerous) subjects, to bring together the *times per single reaction*, placing them in the order from quickest to slowest. In all these cases, a series of stimuli was simultaneously presented, so that overlapping took place.

Performance.	Time in seconds per single reaction.	P.E.
Reading letters or short words	0.33	
Naming colors	0.61	.03
Naming forms	0.89	.09
Adding two one-place numbers (Kraepelin blank)	1.07	.08
Easiest opposites	1.11	.04
Agent-action	1.30	.03
Verb-object, easiest	1.31	.05
Adding 4 (const. increment test)	1.36	.08
Part-whole	1.53	.06
Attribute-substance	1.53	.06
Supraordinate concept	1.54	.07
Action-agent	1.55	.07
Whole-part	1.57	.07
Subtracting 4 (const. increment test)	1.53	.07
Subordinate concept	1.84	.07
Mixed relations	3.14	.13
Adding 17 (const. increment test)	3.90	.31

These differences are of course not to be understood as meaning that the finding of opposites is always, or even on the average, quicker than the finding of wholes when parts are given. The times for opposites that are by no means recondite or unusual run up to an average of at least 5 seconds per reaction. It would be futile to attempt to determine the average or median time for all opposites, and even more futile to make such an attempt in case of the part-whole, genus-species and many other relations; there would be no way of setting an upper limit to the difficulty of the single stimulus words. Such a statement as that the mind passes more readily from species to genus than from genus to species has therefore not much real validity. The fact simply is,

as far as our results are concerned, that the easiest opposites are easier than the easiest part-whole associations, etc. ; and by "easiest" is meant, in case of the several logical relations tests, the twenty easiest. The differences between the speed of controlled associations are perhaps mainly dependent on the factor of frequency in past experience, and especially on the frequency of linguistic transitions. Thus, transitions between opposites are frequent in common speech, and many pairs of opposites thus become verbally associated in a high degree. The reproductive tendencies in case of the most commonplace opposites are therefore strong ; and it may also be that the "mental set", or "determining tendency", is better drilled in case of finding opposites than in many other sorts of logical relation.

VIII. THE UNDERSTANDING OF INSTRUCTIONS

As already mentioned (p. 20), a test should not ordinarily be begun till the subject certainly understands the instructions; otherwise the time measured is partly occupied with grasping the problem, and only partly with its execution. Each test should, as nearly as possible, be a test of one sort of performance. But it seemed desirable to attempt to test the ability to understand instructions, and accordingly efforts were made to prepare a test which should give many different sets of very simple instructions, with the object of discovering the subject's speed in apprehending them. After much experimenting, the following were produced, and the test was named the *directions test*. This test should, we believe, be given as a list or continuous test, with rough timing also of the single reactions, so as to get the median as well as the average time of response. The reactions are to be made with a pencil; and the test can very well be made with a time limit as well as with an amount limit.

The conditions which it was sought to meet in the test material are (1) that the motor response should be very simple and quickly performed; (2) that the instructions should be very simple, but varied; and (3) that the instructions should be as concise as possible, in order that reading time might not be the determining factor.

1. *Easy directions test*. Two blanks are provided, of approximately equal difficulty, according to the results so far in hand. The halves are also approximately equivalent.

2. *Hard directions test*. The object here is to complicate the directions somewhat, by calling for conditional and alternative responses, etc. The blank is arranged in the general form of an Ebbinghaus combination test. The instructions are simply to fill in the blank according to the directions in it. The first two or three directions are easy, so as to put the subject on the right track. The remaining units within the blank (except the last) are so chosen as not to be very unequal, with the object of making

the blank available for use with a time limit. It can not, however, be claimed for this test that it is as well worked over and standardized as the others in this series.

Results with the directions tests. Data so far in hand are rather meager, eight subjects having taken the easy directions test in approximately its present form, and six subjects the harder test—all educated adults. The results follow:

DIRECTIONS TESTS.

	Av.	P.E.	A. D.	Range
Easy tests, time in secs. per reaction..	3.60	.28	.92	2.30—5.70
Hard test, time for whole blank	107.6	6.0	18.4	76—134

If the number of reactions in the hard test is counted as 20 (which is approximately correct), the average time per reactions is 5.38 seconds; the reactions are no doubt slower in this than in any other of the tests described in this paper. To judge from the six subjects who have taken both the easy and the hard directions test, the correlation between the two is very high (Pearson $r = +.92$).

• • •


G H

How many cars has a cat?

Show by a cross which costs more: a hat or an orange.

Write any word of three letters.

Cross out the word you know best: fish, broil, matzig.



How many t's are there in twist?

iii

Write 0 after the largest number: 3 86 12

Mark the name of a large city: London, painter.

7

Write any number smaller than 10.

Put a question mark after this sentence

Cross out the *g* in *tiger*.

Write 2 between the two dots: . . —

How many feet make a yard?

Write + over the longest word. It rained yesterday.

Put a dot below this line: _____

Write the sum of these numbers: $\frac{3}{4}$

Make a boy's name by adding one letter to Joh

Make a cross in the circle:



What comes next after D in the alphabet?

Write 7 in the largest square:



Cross out the blackest letter in TEXAS

Write *g* on the egg-shaped figure:



Make two dots between these lines:



Put the sign = where it belongs: 3 + 2 5.

Write here.....the middle letter of *get*.

Put a nose on this face:



Add a cross and make these rows equal:

 x x x
x x x x

Put a dot in the circle, below the center:



Draw a line around the three dots:

Cross out the last word in this sentence.

With your pencil make a dot over any one of these letters **F G H I J**, and a comma after the longest of these three words: **boy mother girl** Then, if Christmas comes in March, make a cross right here..... but if not, pass along to the next question, and tell where the sun rises..... If you believe that Edison discovered America, cross out what you just wrote, but if it was some one else, put in a number to complete this sentence: "A horse has.....feet." Write *yes*, no matter whether China is in Africa or not; and then give a wrong answer to this question: "How many days are there in the week?"..... Write any letter except *g* just after this comma, and then write *no* if 2 times 5 are 10..... Now, if Tuesday comes after Monday, make two crosses here.....; but if not, make a circle here.....or else a square here Be sure to make three crosses between these two names of boys: George.....Henry. Notice these two numbers: 3, 5. If iron is heavier than water, write the larger number here....., but if iron is lighter write the smaller number here..... Show by a cross when the nights are longer: in summer?..... in winter?..... Give the correct answer to this question: "Does water run uphill?"..... and repeat your answer here..... Do nothing here ($5 + 7 =$ ), unless you skipped the preceding question; but write the first letter of your first name and the last letter of your last name at the ends of this line:

IX. THE FREE ASSOCIATION EXPERIMENT

Few procedures in experimental psychology have so richly rewarded their investigators with the possibilities of practical application. In ordinary psychological nomenclature, it is *the* "association" experiment *par excellence*. Within the past seven years it has achieved, and bids fair to hold indefinitely its place in the foremost rank among the methods of individual psychology. The body of work that has gathered about it is probably greater than that about any other single psychological experiment, and it is not surprising that it constitutes one of the best understood, as well as one of the most potentially significant of them.

The preliminary task of standardization is to provide as error-free a method as practicable, but the main object of standardization is to afford a basis for making comparisons between different individuals. An experimental method becomes standardized in the most complete sense when, given a proper technique, it is possible to accurately rate individual records with reference to an empirical scale. None of the "mental tests" possesses this quality to a degree comparable with the free association experiment, within the limits of the English language. This is mainly due to the work of Kent and Rosanoff which established a definite standard of normality for a specific association material.¹ Within the bounds of its application, it would be an impertinence to offer as "standard" any procedure for the free association test other than the one which these authors have developed; our first endeavor will be then to describe this experimental material, and to indicate what seem to be the best methods for its application.

The Kent-Rosanoff experiment consists of one hundred ordinary English words of somewhat varying difficulty, in the order given on the opposite page, and the making of the test

¹Kent and Rosanoff, A Study of Association in Insanity, *Am. Journal of Insanity*, LXVII, pp. 37-96 and 317-390.

requires, according to the subject, from ten to twenty-five minutes.

This experiment was made by Kent and Rosanoff with 1000 normal subjects, and the responses were tabulated to each individual stimulus word. These constitute the so-called "Frequency Tables," and their use is to determine the "value," in terms of its frequency coefficient, for any reaction or series of reactions in a record of this experiment. After each response-word in the tables occurs a number, which is the number of times that the word to which it attaches occurred as a response to the stimulus word among the 1000 observations. This figure, divided by 10, is taken as the "value" of the response. Thus the "value" of the association *table-accommodation* is .1, because it was given by only one of the 1000 subjects; that of *table-chair* is 26.7, being given by 267 of the 1000 subjects, that of *dark-room* is 2.2, that of *music-art* is .7. It is found, then, that different records of the test show marked differences in the "value" or *usualness* of the associative responses. By means of these frequency tables, the proper "value" is assigned to all reactions obtained from the stimulus-words. *Any one wishing to work with this experiment must provide himself with a copy of the tables,¹ which it is impossible to reproduce here.* Some measure of central tendency for all the measures should be taken, and the distribution of the measures indicates the median to be preferable for this purpose to the average, aside from its greater ease of calculation.

The first and foremost datum of the Kent-Rosanoff experiment is an empirical measure of the tendency of the subject's train of thought to move in usual or individual channels; more accurately speaking, along objective or subjective lines. A number of interpretational questions arise in connection with this finding, which seems less correlated with education than with temperament. It is perhaps the best objective correlate of temperament at present to hand, but the matter is a rather complicated one, more suitable for separate discussion. Here need be emphasized only the preciseness and objectivity with

¹ *American Journal of Insanity*, LXVII, pp. 48-90. To be had of G. E. Stechert & Co., New York.

1. Table	26. Wish	51. Stem	76. Bitter
2. Dark	27. River	52. Lamp	77. Hammer
3. Music	28. White	53. Dream	78. Thirsty
4. Sickness	29. Beautiful	54. Yellow	79. City
5. Man	30. Window	55. Bread	80. Square
6. Deep	31. Rough	56. Justice	81. Butter
7. Soft	32. Citizen	57. Boy	82. Doctor
8. Eating	33. Foot	58. Light	83. Lond
9. Mountain	34. Spider	59. Health	84. Thief
10. House	35. Needle	60. Bible	85. Lion
11. Black	36. Red	61. Memory	86. Joy
12. Mutton	37. Sleep	62. Sheep	87. Bed
13. Comfort	38. Anger	63. Bath	88. Heavy
14. Hand	39. Carpet	64. Cottage	89. Tobacco
15. Short	40. Girl	65. Swift	90. Baby
16. Fruit	41. High	66. Blue	91. Moon
17. Butterfly	42. Working	67. Hungry	92. Scissors
18. Smooth	43. Sour	68. Priest	93. Quiet
19. Command	44. Earth	69. Ocean	94. Green
20. Chair	45. Trouble	70. Head	95. Salt
21. Sweet	46. Soldier	71. Stove	96. Street
22. Whistle	47. Cabbage	72. Long	97. King
23. Woman	48. Hard	73. Religion	98. Cheese
24. Cold	49. Eagle	74. Whiskey	99. Blossom
25. Slow	50. Stomach	75. Child	100. Afraid

which it is possible to evaluate an experiment of such intimate and subjective character.

If special circumstances render it desirable, it is possible to employ only a larger portion of the hundred words for determination of the usualness in response, substituting for the remainder, words adapted to the special situation in hand. It would be desirable indeed, if the Kent-Rosanoff experiment were made the framework of all experiments for *Tatbestandsdiagnostik*, the individually significant words being either added to it or replacing some, not over 10 or 15 per cent. of its constituents. To deal objectively with questions of *Tatbestandsdiagnostik* requires a number of precautions in the construction of the special series, the enumeration of which would be out of place here, and which are fully discussed by the investigators of this application of the method.

Unfortunately, determinations of the "median of community" (i. e., the median "value" of the 100 reactions in a record) have as yet been made in only a limited number of subjects. In some pathological cases it would become indefinitely small; the lowest median ever observed by either of the writers in a normal subject is .7. Such a figure would mean that half the reactions of this record were of a frequency below that of the reaction *music-art* quoted above. The other extreme of the range, so far as observed, is 18.2; i. e., half of the reactions in such a record are more common than, i. e., *music-piano*. The general average value of the reactions in the above mentioned records lies not far from 9.0, that is, about the frequency of a reaction such as *mountain-valley*.

The present experimental method is placed under one disadvantage to a much greater degree than other association tests; its material cannot be repeated within an ordinarily practicable time save under greatly changed essential conditions. One can foresee that circumstances may arise in which a comparative study with material of greater extent is desirable. Provision is here made for such material to be available,¹ but with a change in the character of the material comes inevitably a change in the method of evaluation. Beyond the range of the frequency tables

¹ See Appendix, pp. 80 ff.

one must fall back on the quasi-logical system of classifying the associations that was practically the sole means of dealing with such material until the data on statistical frequency were compiled. The proper function of the test, however, is the same as before, and so is the object of its evaluation: the measure of egocentricity in the responses.

There is no need to fully repeat the remarks in a previous contribution regarding the method of evaluation that seems best adapted to these conditions. It is a five-fold classification, including categories termed (1) the egocentric, (2) the supraordinate, (3) the contrast, (4) the miscellaneous or "internal objective," and (5) the speech-habit.¹

For ordinary purposes of comparison, the principal question concerns the number of reactions that fall into the category of the egocentric; and a large or small number of such associations is subject to analogous interpretations with the empirically determined tendency towards common or individualized responses.

¹The definitions and illustrations of the categories may be summarized from the previous paper as follows:

1. The egocentric reactions may be typified by—
 - a. Predicate reactions. *Cloud-ominous, flower-pretty, crooked-line, red-rose, scratch-cat, lion-roar, money-wish, invent-machine, weasel-stealth, beauty-rose, safe-quite, almost-grown, sing-well, never-kill, nicely-very* (including the responses yes and no).
 - b. Responses in the form of proper names. *Citizen-New York, boy-Johnny, mountain-Kearsarge.*
 - c. Reactions interpreting the stimulus word as a proper name. *Eagle-newspaper, park-square.*
 - d. Reaction involving the response of a pronoun. *Hand-you, health-me.*
 - e. Interjections, failures of response or repetitions of the stimulus word.
2. The supraordinate category is confined strictly to the individual-genus order, defined in such examples as, *priest-man, potato-vegetable, lily-flower, cow-animal.*
3. The contrast group is composed, of course, of reactions in which the response meets the opposite of the stimulus and is made up of such associations as, *good-bad, trouble-pleasure, scatter-gather, fertile-sterile*, and the like.
4. The miscellaneous category is composed essentially of the remaining reactions of the "inner" type. It includes about 45% of all associations.
5. The speech-habit group is composed of associations by familiar phrase (*stand-pat*), word compounding (*play-ground*), simple sound associations (*tease-sneese*) and syntactic changes (*high-height*). (*Psychol. Review*, 1911, 18, 229-288.)

The egocentric is the most variable of the five categories, ranging from next to none to more than half of the total reactions in a single experimental series.

With respect to timing the associations, the stopwatch is the almost universal method, and must be accepted as such, until some more accurate procedure is devised. Significant differences are usually coarse enough to be manifested in measures of no greater precision than this. More uniformity in the manipulation is desirable; at present, some operators start the watch on the accented syllable, others at the first syllable, of the stimulus word. The watch should always be stopped at the first indication of response, even if it does cause occasional failure of timing through the subject's clearing his throat.¹

Individual differences in association time should be discussed from the standpoint of the distribution rather than any single measure. The median is rather preferred as a measure of central tendency, though for practical purposes, its advantages over the average are of less account here than in most cases of skew distribution. The presence of many and exclusively long measures happens to be more important here than in most similar series of measures. Jung has proposed a special comparison of the average and median; this is a convenient statement of the distribution, but it is not an index of emotivity, beyond the limited extent to which the association time can be interpreted in this direction.

The usual instruction in the free association test is that the subject shall reply with the first *word* the stimulus suggests to him, but in ordinary practise this is not rigidly enforced, it being sometimes possible to derive elements of special significance from factors that determine the subject's departure from the set instructions. For comparison with the frequency tables, it is readily apparent that the single word response must be rigidly required in the Kent-Rosanoff experiment; in cases of dereliction from this rule, it is the practise of these authors to repeat

¹ Coughing at such times has received some notice as a *Komplexmerkmal*, though it has been sagely remarked that this loses much of its significance if the subject in question has a cold.

the stimulus word at the end of the experiment, in order to obtain a reaction of the required character.

Presentation and response have nearly always been oral in this experiment, and there is good reason to make no change in them. Experimenters differ as to the manner in which they modulate the voice to the test. Some experimenters, as Dr. Rosanoff, speak the stimulus word with a rising inflection, as though asking a direct question, some as an exclamation, as though endeavoring to hurl the subject's "complexes" in his teeth, others in a monotone. There are subjects whose responses might be somewhat affected by these differences in procedure. This is one of the minor features of the test, in which, though uniformity among different investigators is scarcely practicable, the experimenter should at least strive to keep his own procedure constant.

And while it is not proven, it is most probable that the responses are also affected by the personality of the operator. In making fine comparisons between records by different experimenters this fact must be borne continually in mind.

So far as developed, the continuous form of the free association experiment is a method of possibilities rather than of promise. It would probably be capable of performing many of the functions of the discrete form, but there are external reasons why it would not be likely to perform them so well. The problem of standardization would be practically confined to the development of the most significant scheme of evaluation after the pattern indicated by previous workers with the test. It might be possible to employ the scheme of evaluation proposed for the discrete free association experiment, each single word given being allowed to serve as the stimulus word for the next association. So far as normal psychology is concerned the method has thus far dealt very largely with group averages. As a method of individual psychology it may assume a position comparable to its better known congener only as a result of extended and laborious researches.

NOTE: Blanks for all the preceding tests are to be obtained from the C. H. Stoelting Company, 113-125 North Green St., Chicago.

APPENDIX

The following series of 1000 words is intended for general use in the free association experiment. It is a revision of the series employed in the experiments on the practise effects in the test, and is modified in the manner suggested by the experience of this investigation. It is intended to contain 1000 different words, none over three syllables, so far as possible familiar and unambiguous. It is not far from exhausting the total available number of such stimulus words. Ambiguous stimulus words have a special and useful purpose, but not in a test of the present character. The details of the preparation of this list were substantially the same as in the previous list, save in one particular. The division into twenty series of fifty words each is followed here. But the present list also contains the hundred words of the Kent-Rosanoff series, distributed pro rata, five words in each series, and in their actual order of sequence in the Kent-Rosanoff test; otherwise their arrangement in the series is random, save that none occur in the first ten words of a series. In the list as printed, the words from the Kent-Rosanoff series are distinguished from the remainder by an asterisk, and the associations of these words may be evaluated by the frequency tables. In using single series of fifty words it is recommended that a sheet of paper of fifty lines be obtained upon which several records of reactions to the same stimuli may be conveniently noted. Stimulus words not evoking a reaction according to instructions may be repeated at the close of the series; and if a stimulus word evokes, as a response, the word coming next in the series, this word is omitted and given at the close.

The complete list is as follows:

I	II	III	IV
bottle	drink	locust	weary
produce	captain	divide	tooth
rope	cedar	restore	practise
delicate	mischief	tempt	supper
thick	clean	fade	fun
end	also	cheap	pepper
omelet	path	compel	best
expensive	ride	power	heart
cap	salute	baker	island
barrel	grocery	athlete	machine
burglar	bashful	*black	pit
design	true	roof	*fruit
cry	perverse	cradle	return
hip	occasion	certain	marriage
overcoat	nuisance	travel	marsh
freeze	*deep	impress	owl
*table	pinch	daughter	water
lightning	satisfy	gun	summer
follow	tank	book	copper
parlor	hat	barber	beetle
smoke	nourish	natural	statue
stretch	sister	elephant	clothes
tar	*soft	ostrich	oblong
snake	ham	curse	*butterfly
purpose	ugly	*mutton	constable
*dark	age	haste	cloud
unfair	glory	lizard	collapse
ditch	tough	result	solid
tiger	acid	nonsense	number
*music	*eating	index	goose
wicked	crowd	fool	railroad
prefer	discourse	dense	excite
fish	watchful	life	hornet
instrument	indecent	wine	*smooth
guilty	exchange	*comfort	delay
seed	costume	fever	begin
*sickness	style	infirm	cat
crush	trap	comb	asylum
rich	*mountain	spice	knee
hash	drift	starch	tight
unseen	crime	venture	car
death	cover	*hand	*command
umbrella	abuse (v)	pirate	insect
blood	open	brandy	hope
gift	*house	dress	insist
*man	enjoy	pebble	*chair
allow	untrue	adventure	star
sailor	dismay	lip	ice
prospect	unburden	*short	picture
school	again	pint	bind

V	VI	VII	VIII
room	crown	forget	pot
pencil	get	goat	camp
dig	honest	pulse	shirt
indiscreet	vacant	unwholesome	chain
restless	beechnut	attention	adult
simple	splinter	dairy	violin
measure	unbelief	boast	reason
loss	argue	color	excuse (n)
reckless	conflict	chin	roast
flirt	alike	servant	fig
pause	skin	dislike	face
prosper	inside	dead	*red
*sweet	*wish	*rough	common
avoid	hero	fortune	complexion
fresh	scarlet	candy	deserve
real	lamb	perfect	dim
potato	neck	disdain	view
*whistle	clasp	fierce	*sleep
bite	spear	constant	dirt
clear	*river	violent	shoe
eternal	ox	care	slave
jealous	serious	indeed	protect
barn	garter	*citizen	sting
*woman	key	death	funny
persuade	conquest	sparrow	solemn
merit	*white	*foot	little
receive	scratch	over	*anger
above	cool	bother	ramble
conceal	correct	forward	family
revolt	paste	prepare	annoy
*cold	uncertain	establish	confusion
join	pudding	gold	ripe
tender	*beautiful	along	greasy
offense	bacon	cannon	admire
guide	rancid	boat	cup
prompt	fertile	song	insult
floor	dog	*spider	easy
advance	perfume	another	impudent
bundle	toy	irony	*carpet
ignorant	dust	art	decay
*slow	pansy	dove	*girl
blunder	lake	poem	announce
confidence	*window	herald	prudent
knob	vow	*needle	trumpet
flesh	pancake	treasure	convenient
future	cork	sensitive	ask
wart	gay	fog	supreme
market	feast	tunnel	portable
chocolate	gem	remain	before
disease	enormous	closet	alone

IX

accuse
 flame
 clown
 edge
 frost
 rust
 corrupt
 appear
 caution
 poverty
 curtain
 army
 *high
 people
 polish
 *working
 almost
 idea
 cask
 alcohol
 minute
 stain
 nurse
 *sour
 chapel
 plant
 sky
 forest
 companion
 repeat
 against
 finish
 sermon
 *earth
 conceit
 crack
 drag
 condemn
 iron
 emperor
 plead
 apart
 ivy
 *trouble
 mouse
 event
 claw
 ingenious
 minister
 impose

X

crab
 cart
 riot
 preserve
 preach
 unclean
 steep
 master
 sulphur
 laugh
 imp
 invite
 raisin
 minnow
 promise
 ungracious
 good
 *soldier
 mask
 money
 doll
 rotten
 *cabbage
 cost
 irksome
 apricot
 stone
 *hard
 brute
 escape
 gain
 trifle
 *eagle
 admit
 snow
 about
 equal
 brown
 late
 adore
 perish
 tomb
 harsh
 *stomach
 wasp
 unripe
 friend
 taste
 joke
 propose

XI

uphold
 pickle
 food
 raven
 unwell
 ready
 away
 blame
 competent
 mark
 improve
 *stem
 raw
 defy
 brook
 vile
 storm
 refined
 thankful
 fast
 tremble
 center
 *lamp
 saddle
 pin
 denounce
 cook
 fraud
 bring
 paint
 nut
 immense
 *dream
 condition
 descend
 splash
 abroad
 *yellow
 deceive
 bride
 worship
 infamous
 drop
 *bread
 backwards
 pattern
 cliff
 level
 body
 elevate

XII

intimate
 able
 suspect
 barley
 attack
 dishonor
 accident
 betray
 door
 prince
 *justice
 aim
 revenge
 active
 purple
 decoy
 noise
 fable
 unsafe
 fame
 strength
 scoff
 humble
 *boy
 interest
 old
 wealthy
 modest
 *light
 fact
 violet
 appetite
 attraction
 *health
 across
 piano
 least
 salmon
 price
 garden
 scar
 burn
 ashamed
 *bible
 deny
 quantity
 idle
 wash
 reproach
 energy

XIII	XIV	XV	XVI
alarm	astonish	contented	find
distrust	vest	praise	broom
dimple	whale	pump	dagger
bounce	outfit	poem	love
twig	recover	tennis	try
indulge	embrace	guard	lazy
run	devil	cake	arrow
agree	game	calm	come
secret	towel	remorse	success
ache	faithful	play	coy
advantage.	dwarf	mouth	pure
napkin	use (v)	linen	shower
hill	north	*stove	jump
shelter	thread	belt	*bitter
name	rejoice	amuse	uproar
injure	*blue	sign	catch
*memory	disaster	bag	cóntest
finger	keep	concert	empty
emblem	handsome	*long	unhappy
spool	rescue	absent	divine
unfit	audacious	maiden	*hammer
middle	cage	twist	feather
*sheep	honey	false	disorder
outrage	guess	plunge	naughty
accept	disgrace	murder	exacting
low	shark	*religion	abandon
ardent	flannel	magic	*thirsty
*bath	busy	believe	pay
emerald	unmarried	author	incréase .
wagon	angel	oil	*city
stun	hospital	choke	chase
gentle	secure	*whiskey	unemployed
dodge	*hungry	silver	rhyme
*cottage	jewel	noble	map
shock	nice	breast	wretched
cóntrary	carve	person	distance
hunt	provoke	influence	playful
sin	alive	magnet	impulse
asleep	*priest	glad	land
exquisite	orange	ink	moderate
sweat	battle	introduce	velvet
change	tube	profane	mix
*swift	*ocean	winter	parent
éxpert	apology	help	*square
quality	inch	repress	séparate
instant	pretty	*child	sonnet
progress	brick	field	trade
melt	verse	egg	nest
applause	*head	rat	fancy
cream	bad	mock	bench

XVII	XVIII	XIX	XX
grief	decorate	contrast	demon
parsnip	chance	unhurt	credit
purse	sack	fix	frolic
unlikely	scold	interval	include
walk	portly	fond	rascal
hod	sorrow	grain	pardon
comrade	month	mistake	soap
thought	painful	front	fear
lemon	quarrel	quart	arise
refuse	flower	lecture	cane
paper	suffer	*moon	destroy
cause	fault	usher	chart
pocket	*joy	brier	refresh
task	cab	fountain	*street
kit	discord	church	wrong
*butter	sponge	attempt	rattle
pie	mother	feed	*king
strong	*bed	tame	send
*doctor	den	medicine	glow
pig	support	glove	raft
punish	conscience	dispute	mercy
regiment	devotion	*scissors	dinner
walnut	difficult	evil	*cheese
weather	adorn	irritate	bless
*loud	immoral	advice	drive
remove	spite	neighbor	scorn
fling	brave	cravat	enter
compare	circle	*quiet	scorch
queen	lettuce	entire	expression
same	ivory	contempt	*blossom
war	urge	talk	ghost
play	imagine	touch	aboard
exercise	infinite	flag	parcel
grind	observe	anxious	dreadful
*thief	assist	hurt	small
pretend	beast	*green	oppose
knock	wheat	crumb	queer
orchard	*heavy	escort	reduce
president	repose	notch	reward
decent	terror	bird	outlaw
croak	under	husband	*afraid
plaster	caress	control	drum
lump	lard	stable	clover
question	learn	*salt	intellect
lend	*tobacco	ornament	elbow
around	destiny	errand	milk
merry	fire	hoop	smell
*lion	consent	blush	scandal
awake	*baby	dull	happy
sacred	excellent	many	mill

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